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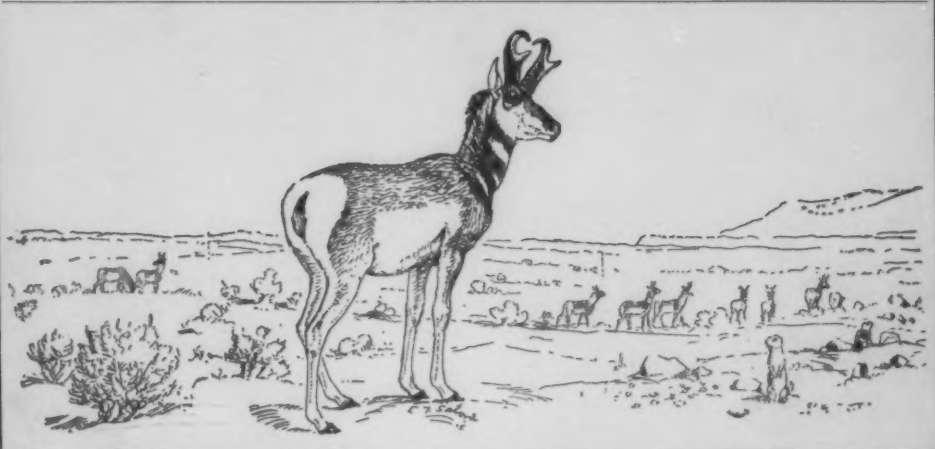

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JOURNAL OF MAMMALOGY

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SOME ELEMENTS OF SEXUAL BEHAVIOR IN PRIMATES AND THEIR POSSIBLE INFLUENCE ON THE BEGINNINGS OF HUMAN SOCIAL DEVELOPMENT.

BY GERRIT S. MILLER, JR.

Recent perusal of some of the more important literature in which the beginnings of human society finds discussion, notably Thomas' *Source Book for Social Origins* (1909), Lowie's *Primitive Society* (1920), Malinowski's *Sex and Repression in Savage Society* (1927), and Brieffault's *The Mothers* (1927), has directed my attention to the way in which even the best informed writers on this subject habitually ignore or misunderstand some of the biological factors common to primates in general which must be taken into account if any knowledge of man is to be gained by the method of comparative study. To give an idea of these factors and to point out a few of their possible implications is the object of this paper.

Lying on the border line between zoology and anthropology, the subject of the beginnings of human social systems can never be adequately understood until all the pertinent evidence brought together by workers in both of these fields has been subjected to detailed consideration. I wish specifically to show that some zoological aspects of sex which appear to be little known to anthropologists are not now given the place that they demand in speculations bearing on these beginnings. To make the subject clear I shall use Doctor Malinowski for rather extensive quoting, partly because his treatment of the subject is interesting and full, and partly because his views are representative of those usually accepted as conforming with the facts.

THE ACCEPTED BELIEF

The matter which I propose to examine in detail is all contained in the fourth part of Doctor Malinowski's book. This part is entitled "Instinct and Culture," and in it the author expounds his conception of some of the psychological characters which are believed to distinguish man from other primates. If they could be shown to be exclusively human, the peculiarities which he describes might be thought to play a crucial part in the process of differentiating the social behavior of man from that of his primate relatives, in initiating human family organizations and the cultural features accompanying them, and in shaping the course which further development was destined to follow. On the contrary, if their existence should turn out to be imaginary some other determining factor would have to be sought.

Discussing "The Family as the Cradle of Nascent Culture," in the second chapter of part four, Doctor Malinowski writes (p. 192): "We shall be able to show that . . . . the family is the only type of grouping which man takes over from the animal. In the process of transmission, however, this unit changes fundamentally with regard to its nature and constitution, though its form remains remarkably unaltered. The group of parents and children, the permanence of the maternal tie, the relation of father to his offspring, show remarkable analogies throughout human culture and in the world of higher animals. But as the family passes under the control of cultural elements, the instincts which have exclusively regulated it among pre-human apes become transformed into something which did not exist before man: I mean the cultural bonds of social organization. We have now to enquire into this transformation of instinctive responses into cultural behavior." It is obvious that before we can understand any process of transformation we must have adequate knowledge with regard to the points of both departure and arrival. For Doctor Malinowski the point of departure in the particular sequence of events which he is studying was determined by the conditions developing out of the association of one man and one woman for the care of their offspring; in other words, from the monogamous family of the simplest type. In his opinion this kind of human association would resemble in some particulars the corresponding associations found among other mammals, but would differ profoundly in its underlying sexual psychology from anything known elsewhere in nature. Since human community life and its attendant culture are regarded as arising solely by extension of the family, all assumptions

that a possible pre-cultural gregariousness or sexual promiscuity among our distant forbears may have influenced the course of institutional development are treated as entirely unfounded.[1] In his own words, "no type of human organization can be traced back to gregarious tendencies" (p. 192). If all of the premises laid down in the first four chapters of this part could be accepted without reserve the arguments as they are presented would not lack force; but unluckily Doctor Malinowski's account of the supposedly unique nature of man's psychology is too largely constructed of elements drawn from the widespread popular misconceptions about the primates with which man is compared; misconceptions which can be seen to arise from wrongly attributing to these animals the patterns of sexual behavior which have become so familiar from observation of domestic ungulates and carnivores that they are popularly supposed to belong to all mammals except man. Even as regards man himself it is difficult to have entire confidence in the conclusions of an author whose opportunities for observation have been so unusual as to lead him to write of human marriage (pp. 231–232): "Thus in the relations between the two consorts the sentiment begins with the gradual awakening of sexual passion. In culture, this, *as we know*, is *never* a merely instinctive moment. Various factors, such as self-interest, economic attraction, social advancement, modify the charm of a girl for a man or vice versa, in low levels of culture as well as in more highly developed civilizations. This interest once aroused, the passionate attitude *has to be* gradually built up by the traditional, customary course of courtship prevailing in a given society." The words which I have italicized will, I think, make my objections sufficiently clear.

The two main suppositions with which we have to deal are as follows: (1) that man possesses a type of sexual psychology which differs radically from that of all other mammals, and (2) that man could have inherited from his primate precursors the tendency to live in family groups but not to live promiscuously.

THE SPECIAL PSYCHOLOGY

It will be most convenient to begin by inquiring into the supposedly unusual psychology under whose influence the "cradle of nascent

[1] In his rejection of the idea of precultural gregariousness or sexual communism Doctor Malinowski is supported by Thomas (pp. 530–531), Lowie (p. 62) and Briffault (vol. 1, p. 195).

culture" is alleged to have taken form. About it Doctor Malinowski writes (pp. 184, 193–201): "The fundamental change in the mechanism of instinctive responses has to be studied upon the very subject matter of our present inquiry: the early forms of family life and the transition between animal and human family. . . . . Let us compare the chain of linked instinctive responses which in animals constitute courtship, marriage and family with the corresponding human institutions. Let us, point after point, go over each link in the love-making and family life of anthropoid apes and ascertain what in human beings corresponds to each. Among apes the courtship begins with a change in the female organism, determined by physiological factors and automatically releasing the sexual response in the male. The male then proceeds to court according to the selective type of wooing which prevails in a given species. In this all the individuals who are within the range of influence take part, because they are irresistibly attracted by the condition of the female. Rut provides opportunities for display on the part of the males and for selection on the part of the female. All the factors which define animal behavior at this stage are common to all individuals of the species. They work with such uniformity that for each animal species one set of data and only one has to be given by the zoologist, while, on the other hand, they vary considerably from one species to another, so that for each species a new description is necessary. But within the species the variations, whether individual or otherwise, are so small and irrelevant that the zoologist ignores them and is fully justified in doing so. . . . . Of what does marriage really consist in animals, especially in apes? Mating occurs as the culminating act of courtship and with this the female conceives. With impregnation the rut is over and with its end there ceases the sexual attractiveness of the female to other males. But this is not the case with the male who has won her, whom she has chosen and to whom she has surrendered. It is difficult to affirm from the data at our disposal whether in the state of nature the higher apes still continue to mate sexually after impregnation. The fact, however, that the female ceases to be attractive to other males while her mate remains attached to her constitutes the bond of animal marriage. . . . . Could an anthropologist provide such a formula for the mechanism of courtship and mating in the human species? Obviously not. . . . . Let us examine this in greater detail. In the first place we see that in man there is no season of rut, which means that man is ready to make love at any time and woman to respond to him—a condition which, as we all know, does not simplify human

intercourse. There is nothing in man which acts with the same sharp determination as does the onset of ovulation in any [other] mammalian female. . . . . Let us inquire now what is the biological value of rut for an animal species and what are the consequences for man of its absence. In all animal species mating has to be selective, i.e. there must be opportunities for comparison and for choice with either sex. Both male and female must have a chance to display his or her charms, to exercise attractions, to compete for the chosen one. Color, voice, physical strength, cunning and agility in combat—each a symptom of bodily vigour and organic perfection—determine the choice. . . . . Rut, however, supplies the animal not only with the opportunities for selection. It also definitely circumscribes and delimits sexual interest. Outside the rutting season the sexual interest is in abeyance and the competition and strife as well as the overpowering absorption in sex are eliminated from the ordinary life of an animal species. Considering the great danger from outside enemies and the disruptive forces within, which are associated with courtship, the elimination of the sex interest from normal times and its concentration on a definite short period is of great importance for the survival of animal species. . . . . In the light of all this, what does the absence of rut in man really signify? The sexual impulse is not confined to any season, not conditioned by any bodily process, and as far as mere physiological forces are concerned, it is there to affect at any moment the life of man and woman. It is ready to upset all other interests at all times; left to itself it tends constantly to work upon and loosen all existing bonds. This impulse, absorbing and pervading as it is, would thus interfere with all normal occupations of man, would destroy any budding form of association, would create chaos from within and would invite dangers from without. . . . . And yet . . . . man does not surrender to his insatiable appetites . . . . he creates barriers and imposes taboos which become as powerful as the very forces of destiny."

For the most part, and in spite of the criticism which I have already made (to which might be added that his tribute to the force of taboo may seem hyperbolic), Doctor Malinowski's exposition of the main features of human sexual psychology appears to be open to no serious objection. It is only when he writes of non-human primates and particularly when he attempts to make specific comparisons between the sexual behavior of man and that of other members of the order that he shows the weakness of documentation which he shares with his colleagues. It seems that the necessarily inadequate story of the sexual life of non-

human primates given by Havelock Ellis nearly thirty years ago[2] is the chief source of information on which he has relied (see footnotes pp. 193, 196); and this account, written at a time when reports on carefully conducted laboratory observations were not available in print, is misleading. Even now pertinent literature is not abundant. The papers which I have been able to find,[3] however, are sufficient to make the entire subject appear in a new light. Their content has so much interest because of its bearing on speculations with regard to the origin of human family life and human sexual customs that I shall offer no apology for summarizing the more important among them. They are here arranged chronologically.

Hamilton, G. V. *Sexual tendencies of monkeys and baboons.* Journ. Anim. Behavior, vol. 4, pp. 295–318. October, 1914.

Doctor Hamilton based his observations on twenty subjects, eighteen macaques and two baboons, kept part of the time in cages and part of the time in a state of semi-freedom, at his laboratory situated "in the midst of a live oak woods in Montecito, California, about five miles from Santa Barbara." The main part of the paper consists of a "list of the types of situations that were arranged by the observer or encountered by the subjects in consequence of their spontaneous activities; and under each description of a typical situation one or more detailed descriptions of typical responses thereto." From the observations obtained in this way we are able to form a clear picture of the sexual behavior of these monkeys; and, in the absence of conflicting evidence about other Anthropoidea, it is not unreasonable to accept this picture as a fair representation of the conditions which may be expected to prevail, possibly with unimportant modifications, throughout the group. Its main outlines are as follows. The females were not subject to a psychological oestrous cycle, but were ready to accept the male at all times when not physically incapacitated (as by traumatism or recent parturition). The males, far from having their sexual responses solely and automatically released by the female when in one definte and special physiological condition, or having their sexual interests fall into abeyance outside of a season of rut, were sexually attracted by any adolescent or adult female at any time; their sphere of sexual interest was not limited to female individuals of their own kind, but was so widely

[2] Studies in the Psychology of Sex, 1, pp. 49–109, 1900.

[3] In my search I have received valuable aid from Mrs. Margaret C. Lewis of the Institute of Psychology, Yale University.

extended as to include females of other species, also monkeys of the male sex and animals of the most varied sort (snake, kitten, puppy, fox, human infant, man's hand); auto-erotism occurred, but not frequently, the only observed instance being in a male kept in solitary confinement. The same absence of specific and single release of the impulse was seen in the females. Among them homosexuality was not absent, though it was less frequent than in the males; one female went out of her way to establish sexual relations with a male dog, another offered herself to a ranch hand employed about the laboratory. Continuous confinement of one male with one female resulted in a marked diminution of sexual enthusiasm in both, particularly in the male, a condition which the animals sought to remedy by special stimulations; vigor was immediately restored by supplying each with a new mate. Females and immature males deliberately employed sexual artifice to turn aside the aggression of a stronger animal.

Montané, Luis. *Notas sobre un chimpancé nacido en Cuba.* Mem. Soc. Cubana Hist. Nat. "Felipe Poey," vol. 1, pp. 259–269. 1915. Separates issued, repaged, 1–17 (without reference to the original publication) under the title, *Un chimpancé Cubano*, "El Siglo xx" publishing company, Havana, 1915. Translation by C. S. Prossy, Journ. Anim. Behavior, vol. 6, pp. 330–333. August, 1916.

A chimpanzee was born on April 27, 1915, at the "Quinta Palatino" park near Havana, Cuba. The notes on this event assembled by Doctor Montané include a short but very significant passage concerning the sexual activities of the parents. The question of rut is not specifically alluded to but the female is represented as having been ready to respond to the sexual advances of the male, "frequently" and "at any moment,"[4] throughout pregnancy as well as before.

Kempf, Edward J. *The social and sexual behavior of the infra-human primates with some comparable facts in human behavior.* Psychoanalytic Rev., vol. 4, pp. 127–154. April, 1917.

Unlike the monkeys observed by Doctor Hamilton those studied by Doctor Kempf were kept under unnatural environmental conditions in cages. Of the six subjects all were *Macacus rhesus* and all but one were males. The female was immature and less sexually attractive to the males than the adult individuals of their own sex. Hence the observa-

[4] In his second paper (1928) Doctor Montané is more specific: "L'acte génésique est répété dix ou douze fois par jour; il a lieu aussi la nuit" (p. 9).

tions bear mostly on homosexuality, and the conclusions pertain chiefly to the explanation of human so-called abnormalities as the result of phylogenetic determinations which can be understood through study of the social and sexual life of infra-human primates. No period of heat was observed in the female; and nothing in the entire paper gives the slightest support to the idea that sexual response in a healthy male monkey is ever in abeyance. One male is described as definitely sadistic.

Sokolowsky, Alexander. *The sexual life of the anthropoid apes.* Urologic and Cutaneous Rev., vol. 27, pp. 612–615. October, 1923.

This paper contains a few observations on some chimpanzees in the Hagenbeck Park, at Stellingen, near Hamburg, Germany. The troup, confined together in one large cage, "consisted of a large adult male, a few younger females and a young male." No periodical oestrus was apparent, the adult male practicing "repeated intercourse every day with his females."

Köhler, Wolfgang. *The mentality of apes.* Translated by Ella Winter from the second (revized) German edition. London, 1925.

In the preface the author explains that this book gives the results of his studies in the intelligence of apes at the anthropoid station in Tenerife during the years 1913–1917. The group of subjects consisted of nine chimpanzees, most of them immature. Their sexual behavior is briefly described on pages 313–315. Nothing of great importance was observed, though homosexuality among the females appears to have been well developed. During the menstrual flow which occurred in three- to six-day periods at intervals of thirty to thirty-one days the sexual instinct was entirely quiescent.

Gear, H. S. *The oestrous cycle of the baboon.* South African Journ. Sci., vol. 23, pp. 706–712. December, 1926.

The subjects were five females and one male, all identified as *Papio porcarius*, received at the Anatomy Department, University of Witwatersrand, at various times during 1925 and 1926. Concerning their behavior the author records (p. 709) that: "During the dioestrus and metoestrus periods the behavior of the baboons shows all the liveliness and vigour characteristic of the primates. They are continually on the alert, showing a restless interest in their toilet, their food, and their

environment." Though the baboons were always on friendly terms with each other, yet, at dioestrus and metoestrus, the females would not tolerate any sexual advances of the male. "During the period of the genital enlargement, however, a marked change in behavior occurs. . . . . In the females under observation, it was at this time that they permitted and even solicited the advances of the male, resulting in frequent acts of coitus." His conclusion is that the [female] baboon shows a typical phase of desire occurring during the engorgement of the external genitalia (p. 710).

These notes are of particular interest on account of the fact that they furnish the only record that I have been able to find which would point to the occurrence of a period of rut in any primate. Nothing of the sort occurred in the female baboon ("Grace") studied by Doctor Hamilton; but from the brief description of this animal (p. 298) it is obvious that she could not have been a *Papio porcarius*. Grace was allowed to run freely with the males, while the females at Witwatersrand were confined separately, in pairs, the male occupying a cage by himself (p. 711). This difference in treatment of the animals may have had some influence on the behavior observed.

Passemard, E. *Quelques observations sur des chimpanzés.* Journ. Psych. norm. et path., vol. 24, pp. 243–253. March 15, 1927.

Further observations on the chimpanzees described by Doctor Montané in 1915. They relate for the most part to activities other than sexual. As of interest from the present point of view may be mentioned, (1) the violently sadistic attack of a male on a female, (2) sexual interest on the part of this same old male in an unripe female ($4\frac{1}{2}$ years), and (3) the seemingly definite stimulation of another male by the sight of a particular woman, this last observation perhaps supplying the complement to the history of Doctor Hamilton's female macaque and the ranch hand.

Yerkes, Robert M. *The mind of a gorilla: Part II. Mental development.* Genetic Psych. Monographs, Worcester, vol. 2, no. 6, pp. 381–551. November, 1927.

The second part of Professor Yerkes' study of "Congo," the young female *Gorilla beringei* captured by Ben Burbridge and brought alive to his home in Jacksonville, Florida. On pages 520–522 are some notes on the beginnings of sexual behavior in this animal, believed to be then

in her sixth year. The subjects were dogs, a male and female. The gorilla always assumed the active part in this sex play, the two dogs showing themselves either indifferent to it or annoyed by it.

Montané, Louis. *Histoire d'une famille de chimpanzés. Etude physiologique.* Pp. 1–21. Paris, 1928. (Probably a reprint from Bull. et Mém. Soc. Anthrop.)

This is a detailed account of the Quinta Palatino (Abreu) chimpanzees. It contains much information of general interest but nothing essentially new as compared with the earlier papers of Montané and Passemard.

Bingham, Harold C. *Sex development in apes.* Comparative Psych. Monographs, Baltimore, vol. 5, no. 1, pp. 1–165. May, 1928.

This very elaborate and important paper is primarily based on observations of the behavior of four young chimpanzees (two males and two females) at the Institute of Psychology, Yale University. Additional material was obtained at the Abreu colony near Havana. Its principal value lies in the day to day study of the first manifestations of sexual behavior in immature non-human primates, a subject on which no detailed information has hitherto been available. The following quotations will give an idea of the manner in which these manifestations grow out of earlier non-sexual activities. Doctor Bingham writes:

> The behavior of these young chimpanzees prior to December 10, 1925, included varieties of social adjustment having, to all appearances, no immediate sexual significance. In these adjustments, however, there appeared components of behavior which differ little, if at all, when appropriately united, from essential adjustments involved in copulatory relations. What appeared to be a new synthesis of responses was observed for the first time on December 10 and recognized as copulatory play. It seemed to involve no new elements in behavior although the adjustment as a whole was novel. Existing factors, previously expressed and developed as parts of other syntheses of responses, appeared to undergo reorganization while I looked on and witnessed initial adjustments of a copulatory nature (p. 78). . . . . These young animals merely repeated over and over the copulatory adjustments without copulation. The relations, it seems to me, may quite properly be termed copulatory play (p. 97). . . . Repeated observations involving both males have convinced me that sexual manifestations are asserted in situations which elicit various kinds of excitement, including both agreeable and disagreeable experiences (p. 92). . . . . This copulatory play, years before reproductive maturity, is the most surprising behavior I have ever observed! There was remarkable coöperation between the pair. The functional adequacy of their behavior is astounding. . . . . I never expected to see such pre-adolescent

behavior. Moreover, I am surprised at the front to front position. So far as I know, the lying, ventroventral position in the anthropoids has never been reported (p. 85). . . . . Presently they took the standing position of quadrupeds (p. 86). . . . . Following the initiation of mutual sex play, there were many repetitions and variations. Conspicuous differences appeared in ventroventral and dorsoventral adjustments. Other variations of significance were noticeable in the social situations that fostered copulatory play. Moreover, the variability that occurred in the sexual behavior itself appeared to be only a continuation of that which prevailed in the sources of such behavior. A heterogeneous group of variable activities, including both primitive responses and recent acquisitions, seemed to contribute in varying degrees to these sexual foci. Excitement—revealed by romping, teasing, petting, fleeing, eating, fighting, tantrums, and commonly by mixtures of these and other activities—was a consistent forerunner of sexual responses (pp. 155–156).

In reading Dr. Bingham's article the passages which I have quoted from Doctor Malinowski should be constantly remembered,—also the popularly accepted opinion thus expressed by Thomas (Source Book, p. 531): "But as he [man] came into possession of a characteristic human mind . . . . he began to make the sexual interest a play interest, and this animals have never done. They have a pairing season, and man has not."

Elements of behavior described by Doctor Bingham but not mentioned by other writers are auto-erotism in females (pp. 148–151), and exhibitionism and social influence (pp. 14–17, 127–139).

Hartman, Carl. *The period of gestation in the monkey, Macacus rhesus, first description of parturition in monkeys, size and behavior of the young.* Journ. Mamm., vol. 9, pp. 181–194. August, 1928.

Doctor Hartman's exact observations on the period of gestation were made possible "by the fortunate circumstance that, as a rule, the females will accept the male whenever given the opportunity." Continuing, he says (p. 185): "We have at least a hundred observations on this point. Some females have, however, consistently refused the male, and certain ones have an antagonism to one or the other of our males, but the stage of the menstrual cycle has nothing to do with their refusal or acceptance."

These published observations are confirmed, as regards oestrus at least, by Dr. George W. Corner in a letter dated January 26, 1928.[5] He permits me to quote as follows: "Combining Hamilton's study with a little information obtained from my monkey colony, I believe the

[5] Doctor Corner's paper entitled "Oestrus, ovulation and menstruation," Physiol. Rev., vol. 3, pp. 457–482, October, 1923, contains much information of interest in connection with this general subject.

situation with respect to oestrus cycle is much the same in *Macacus rhesus* as in the human species. I have tried mating only one female, which was apparently willing to mate at all times of the cycle. In the two primates mentioned, if we speak of oestrus at all, we must consider it diffused over the whole cycle." Further confirmation has been verbally given by William H. Blackburne, head keeper in the National Zoological Park. This able observer, whose acquaintance with captive primates covers a period of about fifty years, tells me that in his experience all monkeys differ conspicuously from other mammals in the absence of periodic "heat." Both sexes are ready for sexual activity at all times; and any reluctance on the part of a female arises either from fear or dislike as regards some particular male or from physical indisposition.

The divergence of fact from the generally accepted idea of "rut and mating" among apes must now be sufficiently obvious. Between the one and the other there is not the slightest resemblance. When, however, we compare the real sexual behavior of monkeys with that of man the great discrepancies vanish. Nothing more is needed than a cursory acquaintance with the things which openly take place in the society to which we belong, supplemented by a slight knowledge of human "cryptadia," to enable anyone to see the parallelism. So far as they have been recorded in the laboratory the main features of simian sexual tendencies tally with well known elements in human behavior; perhaps the only differences between the sexual life of man and that of other primates which can be reasonably considered as radical enough to have had any determining influence in shaping our peculiarly human institutions are these, that man appears to have developed two[6] significant new features: a socially effective sentiment of love,[7] and the physical and

[6] Passive algolagnia (masochism) may have to be added to the list of the more specifically human elements of man's psychology. It can not be found in developed form among any of the accounts of non-human primates thus far published; but Doctor Bingham's observation that sexual manifestations are sometimes asserted in situations which include disagreeable experiences may point to its germ. The supposedly human aberration known as exhibitionism is recorded by Doctor Bingham (pp. 131–132) as occurring in its more simple aspect in both chimpanzees and monkeys, while the type described by Casonova (Mémoires, Garnier ed., vol. 4, p. 406, vol. 5, pp. 81, 91) was observed (pp. 14–17) at the Abreu colony in a pair of baboons and a pair of black apes. I have seen it in the Barbary apes at the National Zoological Park, Washington.

[7] This ambiguous term is here restricted to the complex sentiment defined by McDougall as "tender passion" or "sex love," as distinguished from the simple biological reaction of one sex to the other. (Outline of Psychology, 1923, p. 425, and Outline of Abnormal Psychology, 1926, pp. 162, 560).

psychological possibility of rape. The first of these may, indeed, be less specifically human than the customary absence among laboratory primates of anything more than its most rudimentary features might cause it to appear. The elements from which it could be derived may probably be recognized in the phenomena of individual dominance and favoritism noticed by both Doctor Hamilton and Doctor Kempf, and in some features in the behavior of the young chimpanzees at New Haven (see especially p. 93 of Doctor Bingham's paper), while an instance of the monogamous attachment of a male macaque to the first female with whom he was mated is described in an article by Dr. Otto L. Tinklepaugh in this number of the JOURNAL (pp. 293–300). It is well known that a highly developed type of sex love is often attributed to mammals other than primates, especially to some of the carnivores, but the absence of carefully conducted experiments and of convincing field observations leaves this subject open to legitimate doubt.[8] The conclusion is therefore justified that, although the complex sentiment of sex love may exist in other mammals, in man alone is it known to be sufficiently strong and sufficiently general to require consideration as one of the factors which have greatly influenced social behavior. That rape, on the contrary, is an exclusively human character seems to be beyond serious doubt. I have been unable to discover a trustworthy record of its occurrence in any mammal except man. Its possibility may be regarded as one of the anatomical by-products of the upright human position, a consequence of the remodeling of the pelvic region in such a manner as to do away with the necessity of willingness and coöperation on the part of the female during sexual intercourse. The anatomical need for this kind of coöperation obtains in all quadrupedal and imperfectly bipedal mammals; and the monkeys studied by Doctor Hamilton and Doctor Kempf, as well as the chimpanzees observed by

[8] Human-like association of male and female, together with the psychological conditions which accompany it, seem to occur mostly in mammals whose young are born in a state which places difficulties in the way of their care by the female alone—mere helplessness of the young is probably not the determining factor. A litter of young wolves needs the attention of both parents, a litter of young opossums does not; a young monkey clings to the female and can be adequately reared by her sole ministrations; the help of the male is not required, and I have been unable to find convincing evidence that a true family bond is established. The absence of this ability of the infant to cling to its mother may be regarded as one of the specifically human elements which in pre-cultural times could have combined with more generalized primate characteristics to lay the foundation of the family system in man.

Doctor Montané, Doctor Passemard, and Doctor Bingham presented no exceptions to this rule. Bullying and other attempts at sexual coercion on the part of males were not lacking; but this procedure never led to the desired result unless it succeeded in inducing the victim to coöperate. In such cases, however, we are not dealing with rape, into which, by the definition of the term, the element of coöperation can not enter in any form; the nearest analogue in human affairs is to be found, as Doctor Kempf has pointed out, in prostitution, which "is essentially the giving of sexual favors for economic advantages and physical protection"—here the cessation of bullying. Even could it be shown that true rape can and sometimes does take place in non-human primates its occurrence would probably be so exceptional that it could have little or no influence on the habits of the species as a whole.

It thus appears that the sexual psychology of man, instead of providing the student of human culture with a varied assortment of specifically human elements on which to build, gives him the group of habit patterns common to all primates, *plus 1*, an unusually developed tendency—subject to much individual variation—to form long-continued associations of one male with one female, and *plus 2*, the realization on the part of both sexes that male force is physically so effective that it can accomplish its object irrespective of female consent. Not until the entire subject shall have been reconsidered with these facts and their far reaching consequences to human society kept firmly in mind will it be possible to estimate how much our culture owes to elements of sexual psychology which man himself has brought to it.

THE PRIMITIVE HORDE

Turning now to the idea of pre-cultural human gregariousness and the possibility that our remote ancestors may have lived in sexually promiscuous bands, "primitive hordes," or "Cyclopaean families," we find that Doctor Malinowski makes the rejection of such notions a matter of cardinal importance. "Until it has been shown" he writes (p. 185) "that the assumption of pre-cultural gregariousness is entirely unfounded; until a radical difference in nature is shown between human sociability, which is a cultural achievement, and animal gregariousness, which is an innate endowment, it is futile to show how social organization develops out of early kinship groups." On an earlier page (159) and in another connection he declares that "psychoanalysis has discarded the hypothesis of primitive promiscuity, group marriage and sexual communism, and in this it has the full support of competent anthropolo-

gists."[9] For him, however, animals (presumably he means mammals) possess no herding instinct; some of them have "collective behavior," but in none is there a specific instinct to act in common. "Thus man could not have inherited a gregarious *instinct*, which no animal possesses, but only a diffused 'gregariousness' " (p. 187). Putting aside all question of the validity of this rather fine distinction we can see that the real point at issue is this: have present day human family organizations and the cultural elements which are grouped about them been definitely influenced by psychological factors that could have been inherited from the kind of sexual behavior which recent observation shows to be frequent in non-human primates? We can not now expect to find any race or tribe of people whose culture is so primitive that direct study of it will give the answer to this inquiry. No living race of man is primitive in the zoological sense; all present day races, considered as members of the genus *Homo*, are highly specialized. Hence the uselessness of searching among them for one which might be supposed to be still existing under social conditions which represent the unmodified reflection of primitive man's mentality. Indirect evidence on the subject, however, appears to be more available than might be supposed. Laboratory observations have shown how largely the sexual tendencies of monkeys coincide with those of man in characters which are not primitive and common to mammals in general, but are aberrant, and, it may be, peculiar to members of the order Anthropoidea. When we go further and look into the life histories of the Old World monkeys and apes as revealed by field studies we find that existence in loosely organized bands is the general rule. This type of association is well known to occur in gibbons, baboons, macaques, langurs, proboscis monkeys, guenons, and guerezas. Apparently the same is true of the great apes, though the difficulty of making exact observations on these animals in their native haunts has interfered with the collecting of reliable evidence.[10]

[9] Doctor Lowie says (Primitive Society, p. 62): "Sexual communism as a condition taking the place of the individual family exists nowhere at the present time; and the arguments for its former existence must be rejected as unsatisfactory." In the first volume of The Mothers (p. 195) Mr. Brieffault declares that "It may also be confidently concluded that the human group did not develop out of the animal herd." These opinions appear to be in complete agreement with currently held views.

[10] Field experience in West Africa led E. Reichenow to the conclusion that the gorilla is monogamous and left him in doubt as to the marital customs of the chimpanzee (Sitzungsber. Gesellsch. naturforsch. Freunde, Berlin, 1920, pp. 1–40); it convinced Dr. Neville A. Dyce Sharp that "the gorilla is fiercely

Nothing that I have been able to find in print concerning the behavior of any non-human primate, either monkey or great ape, would justify the assumption that the sexual tendencies of the members of a free band in the forest differ essentially from those which Doctor Hamilton observed (p. 306) in his macaques roving at large among the live oaks at Montecito.[11] Life in sexually promiscuous bands can therefore not be dismissed from the possibilities of ancestral human conditions on the basis of known evidence derived from the habits of primates in general. On the contrary, the common possession by men and monkeys of a type of sexual behavior which perfectly harmonizes with the needs of promiscuous life throws a heavy burden of proof on those who insist that the forerunners of existing men lived under a group formation totally different from that which appears to be the prevalent one among non-human primates. Furthermore, when human sexual behavior is looked at as it is and not as it is conventionally supposed to be we have little difficulty in detecting beneath the surface of the cultural structure unmistakable traces of the framework of the promiscuous horde.

Every society, even the most simply organized that we know, derives its specific character from some particular system of taboos, of thou-shalt-nots, each of which, by the fact of its existence, shows that the integrity of the system in question is perpetually threatened by propensities of men and women to behave in a manner not officially regarded as "right"; and the conventionally "wrong" ways of doing are commonly and properly recognized as arising in large measure from the promptings of untamed nature. What this untamed nature, in other words our inheritance from precultural conditions, may have once been, can, without much doubt, be known in its broad outlines from the taboos to which it has given rise. In no other department of human activity are the taboos more complicated and more strict than in that of the sexual relations. Some idea of the seeming exhaustlessness of human

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polygamous" and the chimpanzee "usually monogamous" (Proc. Zool. Soc. London, 1927, p. 1007). Both gorilla and chimpanzee are said to live habitually in troups made up of individuals of both sexes and all ages.

[11] The objection might be made that observations on captive animals are not always reliable guides to the understanding of behavior under natural conditions. Obviously a laboratory is different from a forest and the sum total of any animal's activities in the one must be different from that in the other. But in neither case can these activities be anything else than the creature's responses to the conditions that surround it. The responses of individuals to each other and to their environment constitute the subject matter of our inquiry; and information about them is equally valuable wherever it is obtained.

ingenuity in this field may be obtained from Westermark's "History of Human Marriage" or from Dr. Charles W. Margold's "Sex Freedom and Social Control" (University of Chicago Press, November, 1926) with its bibliography of 486 titles. Underlying all of this complexity of detail, however, is one guiding, if unrecognized, idea, namely, that natural tendencies must, in the interest of organized society, be brought under restraint; and when we look at the tendencies toward whose curbing the taboos are directed we find that many of them fall within the limits of that behavior which recent study indicates as normal among non-human primates. The present universal existence of these taboos directed toward the curbing of man's primitive primate tendencies must not, however, be accepted as evidence that such behavior no longer occurs in any given society. Infraction of sexual taboo is, as clearly recognized by Doctor Malinowski, a subject on which it is difficult for the ethnologist to obtain accurate information. So great is the obstacle imposed by the dislike to discuss this matter freely with strangers that our knowledge of it, in so far as concerns societies other than our own, is probably in no sense commensurate with our knowledge of the taboos. We may therefore disregard other peoples and at once turn our attention to ourselves. That the sexual taboos suffer frequent infraction in the social world of which we form a part is a fact so well known that it does not require any demonstration. Much speculation has been devoted to attempts at understanding the reason for the existence of such a state of affairs in a society which has given enthusiastic support to systems of strict taboo throughout the course of known history; but this effort does not appear to have led, thus far, to satisfying and final conclusions. Nevertheless, a partial conclusion whose truth becomes evident in the light of the facts which have just been passed in review is the following: that, just as the taboos are largely directed against behavior which would not be questioned among monkeys, so most of the infractions of the conventional code fit into the picture of generalized primate behavior which has been outlined in the laboratory. It is therefore perhaps not unfair to recognize these infractions as, fundamentally, instinctive returns to the way of the promiscuous band, and to see in a Messalina, an Encolpius, and a Rétif de la Bretonne individuals whose sexual conduct, in its departure from social convention, has followed the ancient biological code unquestioningly accepted as law by Doctor Hamilton's "Kate," "Jimmy," and "Mike."

That the primitive type of behavior characteristic of the pro-

miscuous horde is to be looked for in exceptional men and women only is by no means true. The story of the wide spread "subterranean" horde activities which everywhere exist is a long one and nothing would be gained by an attempt to outline it here. But, in studying our own society, as in studying savages, it is again probably not the anthropologist who is most favorably placed to become familiar with it; the story can not be heard in the laboratory as it may be in the divorce court, the physician's office and the confessional of the Roman church. Two subjects, however, which are not beyond the reach of ordinary investigation, may be briefly alluded to in illustration of the wide spread prevalence of horde behavior. The first is the universally prosperous condition of the "oldest of all professions" in spite of milennial persecution of taboo, law and reform. Prostitution is so universal and so abundant that it can not do otherwise than furnish to a large section of society the opportunity to live a sexual life—temporarily or permanently —in harmony with the code of the primitive horde.[12] So true is this that it may be questioned whether prostitution should not be regarded less as an economic phenomenon than as a direct inheritance from our precultural past, as the survival, in modified and commercialized form, of a social condition which was once necessary to the entire species—a behavioral analogue, so to speak, of the persistence of valves in the human intercostal viens. The second is the incidence of the venereal diseases. All authorities agree in saying: "observe the taboos of monogamy and these plagues will disappear." But in fact they show little tendency to disappear. How far they are from vanishing will be realized by anyone who has the slightest familiarity with medical literature. No detailed review of this literature is required, as the information contained in three publications[13] will suffice.

In volume 9 of *The Medical Department of the United States Army in*

[12] Exact data can not be found in print. For one large city an estimate of nearly *eight and one-half million dollars annual income* from "rentals of property and profits of keepers and inmates" is given in "The Social Evil in Chicago. A study of existing conditions by the Vice Commission of Chicago," 1911, p. 107. Accounts of horde behavior, seemingly based on direct observation, may be read in Victor Margueritte's "Prostituée" (Paris, Charpentier, 1907) and in Albert Londres' "Le chemin de Buenos Aires" (Paris, Michel, 1927). W. W. Sanger's "History of Prostitution" ("New edition," New York, The Medical Publ. Co., 1921) is of interest as describing conditions which existed up to about eighty years ago.

[13] Chosen with the advice of Dr. Charles Whitebread, Assistant Curator, Division of Medicine, U. S. National Museum.

the World War, Washington, 1928, one chapter (pp. 263–310) is devoted to the venereal diseases. From its first section, *Statistical Considerations*, I quote as follows:

> That venereal diseases were responsible for great non-effectiveness and economic waste to the Army during the World War is shown by the fact that, of the total primary admissions to sick report on account of diseases only, numbering 3,500,000, venereal diseases were the direct causes in 357,969 admissions, or 10.2 per cent of the whole. If to this number be added cases reported as concurrent with other diseases, the total reported venereal incidence would be 383,706. Venereal diseases, as a class, stood second among the most common diseases as a cause of admission to sick report for the Army as a whole, and exceeded the total number of men killed and wounded in action by approximate y 100,000. As a cause of loss of time from duty, disregarding the additional time unaccounted for . . . . the venereal diseases stood second only to influenza, the greatest scourge of the war. As a cause of permanent disability, requiring discharge from the service, venereal diseases ranked fourth among the most common diseases, being exceeded in this respect by, first, tuberculosis, second, valvular heart disease, third, mental deficiency (p. 266).
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> The number of cases discovered among inducted men on their physical examination after their arrival at Army camps gives a very excellent measuring stick as to the incidence of these diseases among the young adult male population of the United States (p. 265). . . . . Taking the second million as an index of occurrence, the grand total of venereal diseases was shown to be 56.69 per 1,000 or 5.67 per cent. . . . Venereal diseases accounted for nearly 5.8 per cent of all defects and were the third most important cause of defects found in camps (p. 266).

It is explained that the percentages of incidence in this second million are calculated from data obtained through routine physical examination only. They may therefore be regarded as far too low, because Levin, who made more than 10,000 blood tests on troops gathered from all walks of life at Camp Funston and Fort Riley, Kansas, calculated the proportion of syphilitics alone as probably 16.08 per cent among the white men and 36 per cent among the colored (p. 294). About gonorrhoea Dr. J. Allen Gilbert, assistant professor of medicine in the University of Oregon, and medical adviser, Oregon Board of Health, has this to say in his *True and False Sex Alarms* (New York, Rebman, 1916), p. 53: "We are *perfectly* safe in saying that the majority of adult men have had gonorrhoea at some time during their life. No statisticians put the figure lower than 60 per cent and some put it as high as 90 per cent. So that we are probably safe in saying that from 70 to 75 per cent of all adult men have had gonorrhoea at some time in their life." The importance of these facts as indicators of horde activity is increased by the circumstance that both diseases are recognized as not

excessively easy to acquire. Turning now from statistics to history we may profitably read in Buret's *Syphilis in the Middle Ages and in Modern Times* (translated by A. H. Ohman-Dumesnil; F. A. Davis Co., 1895), the account of the epidemic of syphilis which swept through Europe at the end of the fifteenth century. European society was then displaying a perfectly developed system of monogamous taboo. Had the sexual habits of the people coincided with this sytem the epidemic could not have taken place. Had these habits been avowedly those of the primate horde it is not likely that the course of events would have differed much from what we find it actually to have been.

SUMMARY AND CONCLUSION

Facts which I hope to have made clear are these: (a) that a type of sexual behavior which, as compared with the corresponding behavior of other mammals, is aberrant and specialized in its freedom from strict oestrous control, can be found among primates, man included; (b) that this behavior is suited to the exigencies of life in promiscuous bands; (c) that adequately conducted studies of non-human primates reveal the presence of loosely organized, sexually promiscuous horde life as a type of association frequently occurring among these animals; (d) that human taboos and laws are everywhere directed against promiscuous horde conduct, thus showing a universal tacit recognition of the fact that the tendency toward such conduct always exists as a menace to organized human society; (e) that the sexual behavior of man under the social system of European civilization consists in part of elements which are indistinguishable from the activities of the simian horde.

Conclusions which I would suggest as harmonizing most simply and completely with these facts are as follows: (a) that it is now necessary for writers on the beginnings of human family and social systems to recognize that man does not possess a type of sexual psychology which differs radically from that of all other primates, (b) that it seems reasonable to believe that a stage of simian horde life with its attendant sexual promiscuity lies somewhere in the ancestry of the human social systems which exist today, and (c) that human social systems together with their defects will be best understood when they are interpreted as simian horde life incompletely modified to meet the needs of human culture, in much the same manner that the human body together with its defects has been best understood since it has been interpreted as a horizontal

mammalian body incompletely modified to meet the needs of the upright posture.

Whether the primitive type of group formation persisted until man's line had attained a stage of physical development which would be recognized as definitely human or whether alterations began at a stage which would be regarded as still predominantly generalized primate is a question which has no great practical interest. At whatever period they may have begun, the modifications of the promiscuous primate mode of life which we now find in human societies would probably have arisen under the influence of the two elements of sexual psychology—a socially effective sentiment of love and a realization of the physical possibility of rape[14]—which appear to be the most specifically human, combined with and directed by the growth of that enlarged consciousness of individual ownership in persons (wives, children) and things which must have early come to be one of the distinctive social characteristics of developing man.

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THE SELF-MUTILATION OF A MALE MACACUS RHESUS MONKEY[1]

By Otto Leif Tinklepaugh

This account deals with the self-mutilation of a male monkey after he had formed what was evidently a monogamous attachment for a female of different species and then had another female of his species introduced into the situation. I attempt no interpretation of the monkey's behavior, but try to present sufficient details so that the reader can formulate his own conclusions.

The monkey concerned is a male *Macacus rhesus* named Cupid, which the writer procured from an animal dealer in San Francisco, on January 5, 1924. He was taken to the Psychology Laboratory of the University of California, at Berkeley, where he was kept until the fall of 1927.

[14] Narratives of the kind found in Judges, 19: 24–30 and Samuel 2, 13: 1–15, are everywhere accepted as the record of possible situations.

[1] Acknowledgment is made to the Committee for Research in Problems of Sex, National Research Council, for coöperative support of the work to which the observations of this report are incidental.

At that time he and three other monkeys were brought to the Primate Laboratory of the Institute of Psychology, at Yale University.

When Cupid was first secured he was less than half-grown and was awkward and poorly coördinated. His testes were small and but partially descended. These facts suggest that he was between two and three years old at that time. He showed no signs of previous taming. When he arrived he was placed in a room with a female *Macacus cynomologus*, named Psyche. She was close to four years old, had already been kept at the laboratory for over fifteen months and was more gentle, affectionate, and obedient than is typical of monkeys. Her first menstruation had occurred about a year before the male was secured, and though not fully grown she was considered mature. When the two monkeys were first brought together, Cupid, though of a large species, was both smaller in stature and lighter in weight than Psyche.

When the male was first introduced into the room with the female, she rushed to him, put her arms about him and her lips to his. Then she held him close to her, "cuddling" him and at the same time smacked her lips and made low guttural grunts. In the beginning she dominated over him, dragging him here and there as she wished, and biting him lightly about the head and neck when he resisted.

The monkeys spent most of the first afternoon before an electric heater bulb which was attached to their cage to provide heat and light. Psyche devoted much time to picking through Cupid's fur, during which process she came upon his penis. She examined it visually and manually, and then leaned over with her shoulders away from him and lowered, and with her tail elevated so that her genitals were exposed before him. During this procedure she kept hold of his fur with one hand and attempted to draw him toward her. Cupid resisted her tugging and sat staring at the light. Psyche leaned back to him and bit his face and ear lightly. She then assumed the same position a second and later a third time. Cupid made no response. Psyche then returned to him, renewed her examination of his scrotum and penis, and from this turned to picking through his fur again. During the succeeding two days similar behavior was observed, and on each occasion Cupid seemed entirely unaffected by Psyche's attentions.

Two weeks after Cupid arrived he and Psyche were removed to a cage outside the laboratory building. The inclosure was approximately eight feet square and seven feet high and so situated that its occupants were concealed from passers-by by bushes. A few days later he was

several times seen to mount awkwardly upon Psyche's shanks, clasp her sides with his hands and make several pelvic thrusts. This was with erection but without penetration. On these occasions Psyche took the characteristic copulatory position with her head down and hips erect. Once while this behavior was in progress, she waited for a few seconds and then bent her head back nearly under her own legs so that her weight rested on her shoulders, and in this position pulled Cupid toward her with one hand while with the other hand she drew his penis into contact with her genitals. Cupid persisted for a few seconds longer and then dismounted. He then raised Psyche's tail with one hand and examined her genitals with the other. Almost identical behavior was observed five days later.

During the next twelve months Cupid showed marked physical development. His skeletal length increased greatly and he filled out. During the latter part of the year his milk teeth loosened and were replaced by permanent ones. His testes increased in size, but still had not reached the proportions they were to attain eventually. His attempts at copulation with Psyche became more frequent and the movements involved in them showed much better coördination. Throughout this period, however, his attempts did not result in penetration. Though Psyche also grew considerably during this period, she was now smaller than the male. In spite of this disparity in size Cupid seemed to be quite dependent upon her and would rush to her or attempt to hide behind her when frightened by a strange object or sound. In contrast to Psyche's early aggressiveness toward the male, she now often avoided his unsuccessful and frequently desultory attempts to copulate.

While Cupid was still in this immature stage, he formed a habit which it is well to mention here because of its later significance. As is common with monkeys he quite obviously liked to "show off" before onlookers. I taught him to somersault by putting him through the act, that is, by forcing his head down and then pushing his body forward over it. He took his training in good part, seeming to enjoy the process, and after but few trials would perform at command. During this training process, his face was brought down in close proximity to his hind feet. At this time he first seemed to discover these members, and frequently thereafter he could be seen sitting in the cage playing with his feet. From this behavior he developed the playful, or perhaps exhibitionistic, habit of seizing one of his feet or his tail in his mouth and then whirling rapidly around or somersaulting.

Early in 1925 Cupid was first observed to secure intromission. At times he would merely mount the female, make two or three pelvic thrusts and then dismount. Occasionally the copulatory act would last as long as two minutes, ending with the quick rhythmic strokes that accompany ejaculation. Following this development the frequency of sexual congress between the two monkeys seemed to increase. This was apparently due both to Psyche's more frequent acceptance of his advances and to a stronger or more continuous drive in him. From this time on, the mating relations between the pair of monkeys seemed to be mature and entirely normal.

In July, 1926, Topsy and Eva, young females of the same species as Cupid, were secured and taken to the laboratory. When Cupid was admitted to the room in which they were placed, his hair bristled, his tail stiffened and he began to champ his teeth and pull at his leash. His attitude was thoroughly antagonistic, and it seemed best for the sake of future work with the new monkeys temporarily to defer a more intimate meeting. For this reason, Cupid was led quietly from the room and replaced in the outdoor cage with Psyche. A few days later another outdoor cage was prepared about twenty feet from the one occupied by Cupid and Psyche, and the two young females were placed in it. From the time of their first appearance Cupid showed marked antagonism toward them by lunging against the side of his cage with his hair bristling and his teeth showing.

In October, 1926, Psyche was taken from Cupid's cage and Topsy, who a week previously had menstruated for the first time, was led in. Cupid eyed her from a far corner of his cage. Topsy strolled nervously about the cage, never looking directly at Cupid, but frequently stopping with her head away from him, and with her tail up so that her genitals were exposed to him. After about a minute of this behavior, Cupid leaped upon her, seized her by the small of the back and hurled her across the cage. In an instant he was upon her again, and it was with difficulty that she was rescued. Cupid behaved similarly when the other young female, Eva, was brought into the cage. She appeared nervous and frightened, keeping to the far side of the cage and rushing to a top corner of it whenever he showed signs of moving in her direction. Finally he charged after her, and catching her tail in his mouth, bit off about two inches of that member. Eva was immediately removed from the cage and Psyche was returned to it. Cupid greeted her with marked vocalization, copulated with her, and then spent half an hour picking through her fur.

During the following four months, Topsy was placed in Cupid's cage

on three occasions. Before each of these, Psyche was removed to the cage with the young females, leaving Cupid alone for from two to three weeks, after which Topsy was placed in his cage. Each time he attacked her, necessitating her early removal.

Cupid's persistent antagonism toward the young females, even when he had been sexually starved for from two to three weeks, suggested that he had built up what was virtually a monogamous attachment for Psyche. With that in mind, in May, 1927, I set about trying to "condition" Cupid to Topsy. Each day she was placed in a small ante-chamber to his cage, and at the same time Cupid was fed some special delicacy. Topsy was fearful at first but soon became adapted to the new situation. She would play about in the ante-chamber, paying no direct attention to Cupid, although it was evident that her behavior was largely intended for his benefit. Frequently she would take a position such that her genitals were exposed to his view, and there she would stand for several seconds at a time looking back over her own shoulder into space beyond him. He would sit on the opposite side of the separating screen for as much as an hour at a time watching her every movement. Each of them appeared to avoid looking into the face of the other, but whenever by chance their eyes did meet, there was marked emotional response on the part of both. Cupid would rush at the screen, his fur on end and his teeth gnashing, while Topsy would fly in fear to a top corner of her compartment. After ten days of this general procedure, a muzzle was secured for Cupid. With him thus restrained, the two of them were led in company about the campus for an hour or so daily. Following each of these outings they were left together in Cupid's cage for a few minutes. Apparently feeling the restraint of the muzzle, he made no advances either friendly or otherwise, but instead would sit perched in one corner where he would watch her sullenly. On the fourth of the daily outings on the campus, Topsy was running along in front of Cupid when he reached out and touched her. She scampered away in apparent fear, and he made no effort to catch her. A little later he again reached out and this time seized the fur on her side. This time Topsy crouched in the sex position. He mounted her and made ineffectual attempts to copulate. The two were then returned to the cage, his muzzle was removed, and they were left together. Cupid immediately strutted over to Topsy, copulated with her, and then picked through her fur. Beginning at this time the two were friendly and their sex behavior appeared normal.

Cupid and Topsy had been together for five days when the latter was removed to the cage with Eva, and Psyche was returned to the cage

with the male. Cupid's behavior with Psyche seemed normal except for the fact that he copulated less often with her than he had in the past, and that much of his attention was directed toward Topsy in the other cage.

Topsy was returned to Cupid's cage after two weeks, and Psyche was taken out again. Cupid mounted Topsy and was about to copulate with her, when he jumped away from her and began biting his hind feet much as he had formerly done in play. This behavior was noted several times during the first three days the two monkeys were reunited. On none of these occasions was Cupid seen to complete the sex act. On the fourth day he was discovered with his hind feet rather badly lacerated. To facilitate examination and medical attention, he was led from his cage by the doorway leading into the area between it and the nearby cage now containing Psyche and Eva. He looked back at the cage where Topsy still remained and then toward the cage containing the other two females. Psyche, who had seemed to be much upset by Topsy's presence in Cupid's cage, was now on the side of her cage shrieking threateningly across at the other female. Suddenly, and with no previous signs of anger or particular emotion, Cupid lurched to the end of his chain and began to bite himself. In a few seconds he tore huge jagged rents in his already lacerated legs. Then, as though in intense pain, he jumped into the writer's arms. He jumped down again and this time tore a three inch gash in his hip, ripped his scrotum open, lacerating and exposing one testicle, and mutilated the end of his tail. In all there were twenty newly made lacerations on his body, and of that number only one was in close proximity to his genitals.

As soon as Cupid could be quieted he was muzzled, then his wounds were treated and his four canine teeth were extracted. Psyche was returned to the cage with him to nurse his wounds—a task she performed remarkably well—and also because it was believed her presence would have a quieting effect upon him. As would be expected, Cupid was weak and sick from loss of blood and from the numerous lacerations. He rapidly recovered, however, and after about three weeks seemed to resume fairly normal sex relations with Psyche. During a period of about four months following the time of his self-mutilation, he frequently seemed to be in a state comparable to the depression of some psychoses. Through many hours of a day he would sit quietly in a corner of his cage or in the nest box, with his head drooped, and only occasionally would he observe the happenings about him through the corner of his eye. During these periods he was highly irritable if approached and he absolutely refused to obey commands. At other times he showed

marked hyper-excitability, and would rush wildly about his cage shaking the sides of it and threatening any person or monkey who came in his path.

During Cupid's convalescence there appeared evidence that I was in some manner involved in his abnormal behavior. When greatly excited he tended to seize and bite his still irritated legs or scrotum. The same behavior was called out whenever I appeared and directed my gaze toward him or spoke to him. If I merely passed the cage, or even if I stopped before it without looking at or speaking to him, the biting did not occur. Nor was the presence or speech of other people adequate stimulus for the response. This was verified by numerous trials and always with the results indicated. The same behavior continued throughout the summer and reappeared when Cupid, along with the three females, arrived in New Haven, after having been separated from me for a period of eight weeks.

Immediately prior to and during this eight-week period Cupid was kept caged with Topsy and, according to the reports of his caretakers, his relations with her were entirely amicable during that time. When the group of monkeys arrived in New Haven, Cupid was immediately admitted to the cage with Psyche, whom he accepted and copulated with at once. His tendency to self-mutilation whenever I looked at or spoke to him still persisted. Judging that time alone would not be sufficient to break up this tendency, I first resorted to verbal and physical threats and then finally gave him a severe whipping. There followed a few occasions when a verbal command served to stop the behavior in its incipiency. Then came a period during which my presence would stimulate him to raise an ankle or wrist to his mouth as though to bite it, but instead of carrying the act farther, he would usually turn to picking through the fur on the same member.

As this report is written, some fourteen months after Cupid's first attack upon himself, he is experiencing what seems to be normal sex relations with Psyche and Eva, the other female, Topsy, having been sacrificed after lead poisoning. Cupid's contact with Eva was gained when he broke into a large cage with her and one of the other females during the night. For some two or three weeks he remained antagonistic toward her, but with her increased strength and agility she was able to successfully avoid him. Gradually the barrier was broken down and he accepted her both socially and sexually. Cupid is now in excellent physical condition. He is not as tractable as he was when younger, and he shows considerable irritability, most of which is directed towards four chimpanzees which occupy an adjoining cage. When, in my

absence, he becomes highly excited, he will seize one of his hind feet or the sheath of his penis in his teeth, but he bites them only lightly, and during the past four or five months has inflicted no noticeable injury on himself. The biting of the sheath is apparently not to be confused with masturbation, for neither now nor at any other time has he been observed to masturbate in any manner.

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BREEDING HABITS OF THE CANADIAN PORCUPINE (ERETHIZON DORSATUM)

By Parke H. Struthers

[*Plates 22–23*]

The porcupine is one of the few North American mammals whose habits have not been carefully studied. It has been casually referred to in literature as an animal of interest, yet one to be feared because of its dangerous coat of spines and its devastation of the bark of forest trees. So diverse is the respect for this animal that we find the state of New York and certain parts of Canada protecting it by law, while Vermont and New Hampshire offer a bounty for each porcupine killed. In spite of its being neglected as an object for scientific study, the pine-hog, as it is frequently called, is a very interesting creature. Thanks to the peculiar specialization of its hair and the fact that it is a discreet glutton, feasting on food no other animal wants, the American porcupine group has been able to face the adversities of life since Miocene times (Woodward, 1898).

The scope of this paper deals with the habits associated with breeding, together with a chronicle of the rate of development and the external changes that occur throughout life. As a source of information the writer draws upon personal observations made during the past three years on occasional visits to Nelson, New Hampshire, a hill town abounding with hemlock forests and deserted farms. Within a radius of two miles from the village center, a recent survey, made by John Priest, shows that there are at least twelve colonies, each containing not less than fifty individuals. In this same locality there are also many isolated family groups made up of half a dozen or more animals. The large colonies inhabit rock dumps made years ago by graphite-mining operations, or deep crevices in out-cropping granite. The smaller

groups live in cellar holes and under rocks undermined by the action of water, or by other animals. This section is near the southern limit in which the porcupine is abundant in the state. This observation agrees with the report of Professor Jackson (1922) who says they are fairly common throughout the northern two-thirds of New Hampshire.

Porcupines invariably travel in runways. These connect the dens with the feeding grounds and watering places. In such runways the writer places Oneida no. 2 jump traps, with the result that he has caught over one hundred individuals. Many of these have been taken to Syracuse, where they are kept in a large pen in the attic of one of the university buildings (fig. 2). Here it has been possible to observe the animals closely. Through the interest of vice-chancellor William P. Graham our porcupine family is taken each summer to the shores of the St. Lawrence River, where they can again get back to nature. Doctor Graham has found that, when given a diet of bread and apples, the animals thrive in confinement.

During a large part of the year porcupines, both male and female, live together without any discrimination of sex. In the large colonies the number of animals occupying a certain den seems to depend upon the amount of space available and the physical advantages of the den. The writer has taken fifteen porcupines from the opening into one den within a week and in 1925 nineteen were killed in a den, which has an opening sufficiently large for a man to pass through. The occupants of a den one day may spend the next day in a different den within the confines of the colony. In feeding, an animal follows the main run as far as the feeding grounds and then selects its own tree on which it feeds. The porcupine does not hibernate, so that during the winter the activities of the animal can easily be observed by studying their tracks after a new fall of snow.

RUTTING SEASON

The mating of porcupines begins the first week in November and is generally finished by the first of December. Seventeen adult females taken between November 21 and 25 showed only three that were not pregnant, while twenty-six females taken between December 20 and 25 showed only one that was not pregnant. This is probably the only mating season of the porcupine, for many females taken during the summer have in no case shown signs of pregnancy. The uniformity with which insemination occurs is seen by a comparison of the length of twenty-five embryos taken between December 17 and 31. This data is shown in table 1. All the embryos exceeding 38 mm. were collected

after the twenty-seventh of the month. Fifteen embryos taken the last week of November all measured under 5 mm. The records show no correlation between the size of the embryo and that of the parent.

It seems probable that usually a male mates with but a single female. In one hundred individuals, however, the ratio of males to females is respectively four to five, and this indicates that some of the males mate a second time. In support of this second mating of the males is the fact that a few females are not pregnant until after December 1. There is little or no competition among the males for the female takes the initiative. Examination of thirty adult males has failed to show any scars or other signs suggestive of a struggle for supremacy, while males kept in captivity show no marked difference from the females with respect to domination, except during the breeding season when it is the female that makes the advances.

TABLE 1

LENGTH (C.R.)	NUMBER OF EMBRYOS
mm.	
—5	1
9	1
13–15	3
15–19	4
20–25	3
26–28	3
32–35	2
38–39	4
40–43	2
53–57	2

Rutting is evidenced by a pairing of two individuals during which time they are constantly together. A description of two porcupines that mated in confinement will give the clearest idea of the method. In September (1926) an adult female was placed in the pen (fig. 2) and on January 1 (1927) an adult male was put in with her. For seventeen days the pair apparently ignored each other, but the eighteenth day the female was seen to follow the male about the cage and to sit near him when inactive. The pair remained aloof from the other porcupines in the pen. Occasionally the pair would converse with each other by means of sharp grunts. Eventually the male became very attentive. Frequently they would go through an amusing exercise of rubbing noses. These activities continued until the morning of the twentieth day, when copulation occurred. This lasted for several minutes. The

female stood with the tail held sharply to one side, and the quills on the back lying very flat. The male stood on his hind legs, while the front legs grasped the sides of the female. There was no repetition of the act and it has never been seen to occur among the captive porcupines at other seasons of the year. The delayed mating of this pair was undoubtedly due to factors associated with their being in captivity.

Certain changes in the color of the fur are manifested at the time of mating. All porcupines on reaching the age of maturity have a characteristic brown color on the quills of the head and neck, which increases with the age of the animal. Close examination shows these quills to be very slender and generally pigmented the whole length of the shaft (fig. 3A). They have a close resemblance to the reddish quills of the genus *Coendou*, a genus living in Central and South America (Beddard, 1902). At the beginning of the rutting season there appears on the abdomen of adult females a pair of cinnamon-colored spots two inches long and about one inch wide, and so placed as to include the two mammary glands lying on either side of the mid-line. This color is confined to the hair of the region and represents only a temporary pigmentation for by the first of January it has almost completely disappeared.

GESTATION PERIOD

The gestation period of the porcupine is sixteen weeks. The authority for this statement is based on the record of the captive female that was fertilized January 20 and delivered her young May 13. Observations made by residents of Nelson, New Hampshire, say that the young of the porcupine are born in March, which also confirms the statement.

Female porcupines as a rule carry but one young. Of the fifty-one pregnant females that the writer has examined, only one has contained more than one embryo. This case is of considerable interest, for one embryo measured 17 mm. crown-rump, while nearer the ovary and in the same cornu of the uterus was found a second embryo which measured only 2 mm. Such a variation in the size of the embryos would indicate that the smaller embryo was formed from an egg developing at a later ovulation and fertilized by a sperm retained in the horn of the uterus since the original copulation. This condition can not be considered a normal type of twinning. In spite of the statement of recent authors (Pratt, 1923) that the porcupine rears from one to four young a year, I agree with Cuvier (1863), who reports but one young a year.

As a general rule at the beginning of pregnancy the female porcupine is very fat, and the writer has noticed that the females during December

and January are prodigious eaters, frequently braving severe weather to visit the feeding grounds when the males will not venture out of their dens except during clear weather. On the morning of December 23, after a stormy night, nine pregnant females were taken from a string of eighteen traps, but not a single male or immature animal. As pregnancy advances the females become more and more quiescent and it is during this dormant period that the animal utilizes the energy stored up in its fat, for at parturition the mother is very thin.

THE PORCUPINE AT BIRTH

The observations on the young at birth are taken from the individual born in captivity. This animal weighed 510 grams and its total length was 27 centimeters. Its eyes were open and the tips of the incisor teeth just showed above the lips. Its body was sparingly covered with long grayish-black hairs, especially on the anterior half. On the second day the needle-like tips of the quills could be felt and at the end of a week they were about one-quarter of an inch long. Six hours after birth the porcupine could waddle about the cage and on the second day it climbed to the top of the pen, some six feet from the floor. From the first the infant was extremely wild. Later during the summer Doctor Graham, with the greatest of patience, endeavored to conquer this inherent fear but without success, although the mother became so tame that she would climb up on his knee to be fed. For a week it nursed its mother and then began to eat the regular diet of bread and apples. The most remarkable thing about this young animal was the rapidity with which it passed out of the period of helplessness. Within a day it could flee from danger and in a week it no longer needed food from its mother. On the first day of life it would turn its back to an imaginary enemy and hit with its tail, although the quills were not as yet in evidence.

The captive mother showed almost no signs of attachment for her offspring. Occasionally she would utter a grunt in answer to its cry. On rare instances she would rub noses with her baby. Other adult porcupines in the den entirely ignored the youngster. Sometimes the little creature was removed from the pen, an experience which always brought forth cries and struggling. In spite of the apparent kidnapping the mother remained completely indifferent. Nursing occurred at intervals of about four hours during the first week. While the young porcupine was eating, the parent either sat on her haunches or stood on all fours.

PERIOD OF ADOLESCENCE

At the end of the first summer the young porcupine weighed three and one-half pounds (18 inches total length). This weight corresponds with that of several animals taken during November and December. At this age the quills are almost entirely concealed by long fine hairs of a grayish-black color, which give a characteristic fluffy appearance to the porcupine.

By the end of the second summer the weight has increased to seven or eight pounds (21 inches total length). At this age the animals are sleek and very active, the long hairs are not so pronounced and the white quills with their black tips distinguish the two-year old from the yearling or the adult. Still another year must elapse before the young porcupine reaches maturity, but at this age it seems to feel its potential strength, for it wanders far from the home den. Biologically this may be a valuable incident in the life history of the animal, since it guards against the possible weakening of the race through inbreeding.

The third autumn shows interesting changes especially in the female. The quills now are of two types, the large white spines with black tips and those in the region of the head and neck which are reddish-brown in color and very slender (fig. 3). The sutures of the skull begin to disappear (fig. 4), and the cinnamon-colored markings are seen on the abdomen. The animal now weighs eleven or twelve pounds (25 inches total length) and has reached the age of maturity.

PERIOD OF ADULT LIFE

The number of years a porcupine lives after reaching the age when it can reproduce itself is not known. After the third year the animals still continue to grow until they reach an average weight of fifteen pounds. Two-thirds of the porcupines taken by the writer have weighed twelve pounds or over. The largest female weighed eighteen pounds, while four males each weighed over nineteen pounds and varied in total length from 29 to 35 inches. Adult males average to weigh two pounds more than females. The writer has preserved the skulls of all animals killed and he has found the degree of fusion of the cranial sutures (fig. 4) corresponds closely with the increase in weight of the animal.

Some porcupines live to senescence. From the one hundred individuals referred to in this paper, five showed signs of old age. Each of them weighed over seventeen pounds and had a total length of over thirty

inches each. They were living apart from any colony of porcupines. Two made their homes in large hemlock trees, one lived under the piazza of a summer home, and one resided in a deserted fox den. In each case the quills were very few in number, especially on the rump where the creased gray skin was exposed. The vibrissae about the face and the long hairs on the shoulders were white. The bones of the skull were completely fused, leaving scarcely a trace of the original suture lines (fig. 4A). The teeth in each case were in excellent condition. Each of these graybeards possessed gruesome dispositions and seemed to retain all their vigor and tenacity for life. An animal taken on the morning of December 24 had completely gnawed away four slats, two inches wide one inch thick and two feet long, from the piazza railing and had partially destroyed nine others; he had completely removed the lattice-work over an area two feet square and yet there was scarcely a chip to be found. All this occurred within twenty hours, the time elapsing between tending the trap.

A careful examination showed a remarkable uniformity in structure and size of porcupines of a corresponding age. The chief variations occurred in the color and thickness of the fur. One creamy-white albino was taken from a small family group by A. L. Struthers in the region under discussion and another was reported to have been seen in the same vicinity several months later. Even the claws of the animal taken were of a light amber color; the incisor teeth, however, had the characteristic brownish-yellow shade. The degree of thickness of the winter coat of porcupines varies from that of individuals in which the thick fur does not show beyond the quills, to that of animals in which the quills are completely hidden by a beautiful covering of dull-black hair. This variation is due either to advancing old age, or to the difference in the warmth of the dens. Porcupines that spend the winter in captivity, where the temperature never drops below 50° Fahrenheit, do not grow a thick winter coat. The reddish-brown color on the quills is associated only with maturity, for animals under eleven pounds in weight never show it. Occasionally fully mature individuals will also fail to show this type of quill, especially among the males.

In spite of the fact that porcupines rear but one young a year they increase in numbers very rapidly under favorable conditions. About one mile from the town of Nelson there is an old lead-mine dump covering an area of about two-hundred feet square and surrounded by a young growth of hemlock and hardwood. At the foot of the dump flows a small mountain brook, while across it and up a little slope is an old

apple orchard and a set of dilapidated farm buildings. During the winter of 1917, Charles Osgood, the owner of this land, trapped over one-hundred porcupines, and it is a matter of local gossip that on settlement day he paid a part of his taxes with porcupine snouts neatly strung on wire, which were worth at that time twenty-five cents each as bounty. After a period of ten years, during which time no porcupines were taken from the colony, the writer began trapping and during two weeks in 1926 and one week in 1927 caught forty-two animals. These consisted of seventeen adult males, twenty adult females and seven immature porcupines. Since young animals are more difficult to trap, the number of immature individuals does not represent a fair proportion as compared with the adults. At the present time there are only seven of the original thirty-two dens occupied and the number of animals remaining probably numbers less than twenty-five. Under almost identical conditions the writer took thirty-five porcupines from another colony, located on the Elliot estate, in which the trapped animals consisted of seven immature, twelve adult males and sixteen adult females. The increase in the size of each of these colonies has largely been accomplished by the breeding within each group and not by any appreciable influx from other colonies. For each of these colonies is separated from other groups by at least a mile and the addition of foreign two-year-olds would be offset by those leaving the colony.

With these observations on the breeding habits of the porcupine before us, it is of interest to consider what part they play in the animal's long existence as a resident of North America. Judging from the extended period of pregnancy and that of adolescence the porcupine enjoys a long span of active life. This allows the female to beget several offspring and these over a period of years, which minimizes the danger of all her young being born under unfavorable conditions or at a time when the chances for its survival would be precarious.

The same biological principle that operates in the human family exists in that of the porcupine but with a modification. Namely, the concentration of all the maternal nutriment and care upon one young, so that it will mature and form the scion of the family. In the case of the porcupine, however, the young almost immediately receives its birthright in the form of a complete complement of protective weapons for facing the dangers of life. Maternal care is relatively of little importance among animals like the porcupine where aggression and offensive warfare are entirely unknown. It is very noticable that young porcupines up to the time of maturity are much more cautious,

impossible to make friendly and always on the alert to use their defensive weapons.

There can be no doubt but that the porcupine has an extremely low infant mortality. In fact, the writer knows of no enemy that would injure either the young or the adult porcupine in the vicinity visited, except man. In some places bears and wildcats prey upon this animal, but the young would be the last to fall victim because of its greater amount of caution.

The porcupine holds an unique position in the animal world, and one which to a large measure accounts for its present survival. All the weapons of defense which most animals rely on for their existence, such as, speed in flight, cunning, keen senses of smell, hearing or sight, have been sacrificed for two great specializations, a coat of needles and a set of teeth which are capable of rendering fit for comsumption the bark of trees that no other animal wants. Armed with these the porcupine lives its life among the other animals of the forest, little worrying about the struggle for existence with which they must continually contend. They have nothing he wants and with his coat of spines very few of them can do him harm.

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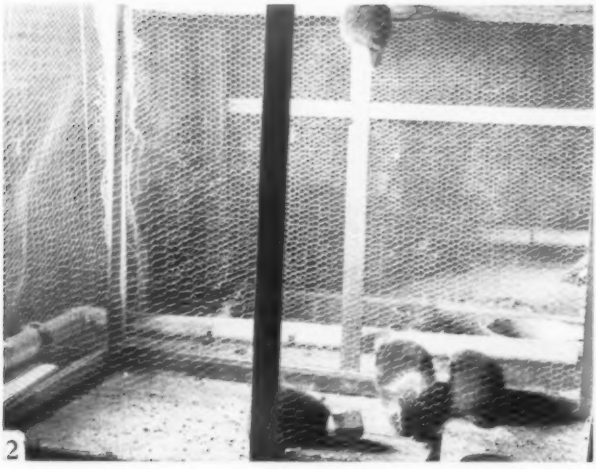

FIG. 1. Adult male porcupine recently caught. Its winter coat conceals most of the quills. $\times 1\frac{1}{2}$. FIG. 2. A pen of five porcupines living in the attic of Lyman Hall. The animal seen near the top of the cage is immature and invariably seeks this position when disturbed.

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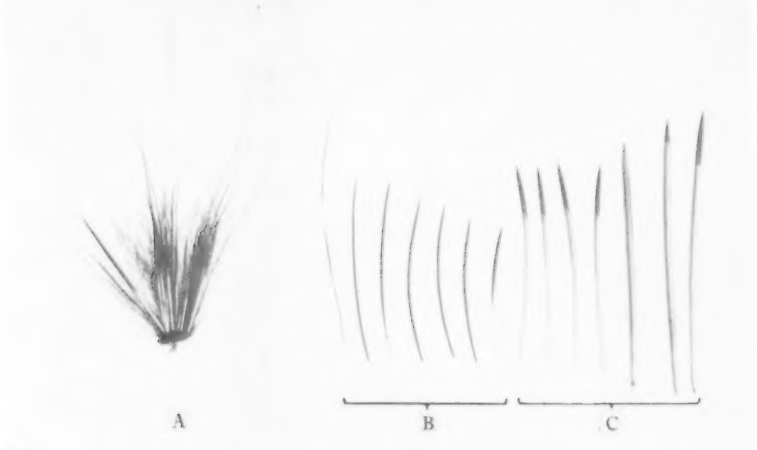

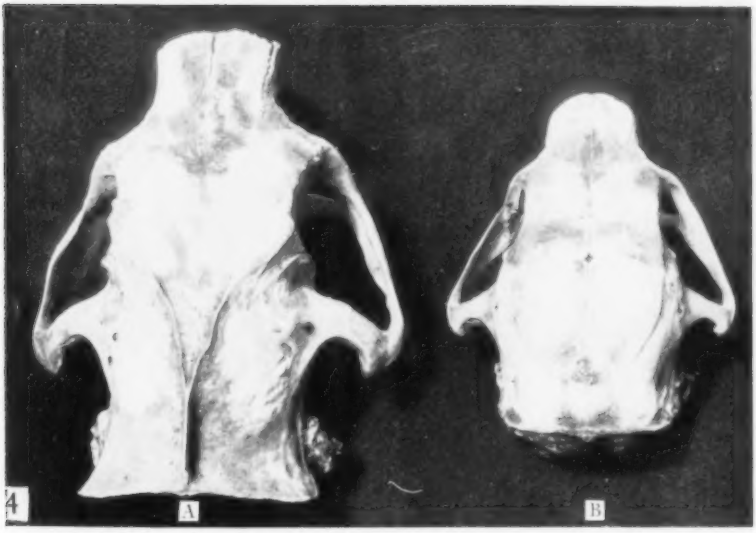

FIG. 3. (A) Section of the fur in the region of the neck. (B) Slender reddish quills found in the region of the neck of adult porcupines. (C) Stiff quills found on porcupines throughout life. The tip is usually shaded brown or black. FIG. 4. Dorsal view of the skull of an adult (A) and an immature porcupine (B).

STATE OF MICH.

THE ELK SITUATION

By M. P. Skinner

The elk (*Cervus canadensis* Erxleben) formerly occurred in every one of our states, except Maine, Delaware, Mississippi, Louisiana, and Florida. In the north, their range lapped over into the southern provinces of Canada; in the south, they reached the northern counties of the gulf states, and were common in New Mexico and in Arizona. The elk were not naturally desert animals, yet they lived on the higher mountains, at least, of all the desert states. An examination of this range will show that they did not like hot areas, nor swampy areas, nor hot and dry areas; but that they adapted themselves to a variety of woodlands, grasslands, and even dry areas, if fairly cool. On the other hand, they were not suited to really cold weather.

This wide ranging is ground for hope that we can save the elk as wild animals. For animals that cover a wide range are those that are most adaptable to a habitat, and that most easily survive there. Such are the white-tailed deer that exist in spite of intensive hunting; the gray wolf and the cougar that still live wherever they have the slightest chance; and the black bear that hangs on in eastern states until Pennsylvania, for example, can afford to allow the killing of hundreds every year. Naturally then, we take courage from these conditions to hope for the elk. In the very center of their range (the Yellowstone region), where the largest band of elk exists today, is the best chance to preserve them. Indeed, we can go even farther and say that if we can not preserve the elk in the Yellowstone country, we can not preserve them anywhere as wild animals.

When the Yellowstone National Park was set aside, it was not a primary object of the dedication act to protect the animals. It was not thought necessary, because they were then abundant throughout the far west. Fortunately, it soon became apparent that there was need of protection and that this region was peculiarly adapted to the purpose. Later laws and regulations of the park made animal protection a feature.

For some time after protection began, the elk did as they had previously done. They left the park for the winter, migrating in all directions to the lower plains where they could get feed free from the deeper snow of the high mountains. But gradually this migration became more and more restricted, partly because of overstocking of the

lower range with domestic animals that destroyed the forage; but mainly because the lower valleys and plains became farm lands and criss-crossed by wire fences difficult and even dangerous for the elk to negotiate. Soon after that, it became evident that the only hope of saving the elk lay in keeping them in the mountains on protected lands, rather than letting them migrate down into the lower valleys fairly swarming with rifles.

Since the high, mountainous country was not a natural elk range in winter, these animals had to adapt themselves to the new conditions, not an easy thing to do in a short time. While trying to adjust themselves, the elk began to dwindle. Back in 1900, when I first knew the region, there were 75,000 elk in it, in 1910 there were still 50,000 elk; but in 1925 there were only 30,000 elk left, and I doubt if there are more than 25,000 elk in the region today. Furthermore this number keeps right on dwindling, until we are faced by the fear that another devastating winter like that of 1919–1920 may decimate the present band.

You will notice I am speaking of all the elk in the Yellowstone region. We are making a mistake in thinking and planning for them in sections, like "the northern herd," "the Jackson Hole herd," "the Gallatin herd" and "the Shoshone herd." We can not differentiate these elk in summer, for they constantly intermingle throughout their summer ranges. So that whatever happens to the elk of "the northern herd" has its effect also on the elk of the Gallatin, Jackson Hole, and Shoshone valleys.

Because these elk are held up there in an unnatural winter habitat, they should be given the best of care and protection. Possibly I go a little farther in this direction than most authorities do; still, our obligations to the elk were very early realized. In 1904, Maj. John Pitcher, then acting superintendent of the Yellowstone National Park, made mention of this duty to feed the elk even during parts of ordinary winters. Later, the three winters following 1908 were severe and the losses of elk were so heavy that the national congress was petitioned for help. An appropriation of $20,000 was made for food and protection. Better yet, Edward A. Preble went to investigate elk conditions in Jackson Hole and later made an able report.[1] Unfortunately Mr. Preble could remain in the country only from March 16 to early June; but his report summarized the little then known of the elk situation and led to the establishment of the Biological Survey Elk Refuge. Since this original

[1] Preble, Edward A. Report on Condition of Elk in Jackson Hole, Wyoming, in 1911. U. S. Dept. Agric., Biological Survey Bull. no. 40, pp. 1–23, 1911.

investigation, much thought has been expended on the situation and many theories and plans for elk preservation advanced.

But most of our care of the elk has been theoretical rather than really practical. To properly care for such wonderful animals, we should know all about them. Actually, we know very little: not nearly enough to formulate a good and successful working plan.

Naturally, food is of primary importance and a thorough knowledge of what an elk likes, eats, and thrives upon is essential. Yet it is only in the last year that we have begun to collect data on what elk actually eat at different seasons. You will readily see the absurdity of trying to provide forage and feeding range for wild animals when we do not know what they eat, much less whether that food grows upon the feeding range in question. But I do not wish to belittle the wonderful efforts of the U. S. Biological Survey in Jackson Hole; the feeding of hay by the National Park Service; the recent, successful efforts of the Izaac Walton League to provide more hay and more range for the elk; or the study and care that the U. S. Forest Service is giving to these animals. All four of these undertakings are exceedingly valuable. They have saved us many elk, and will save many more in the future. They are the best we know how to do now, but are actually only stop-gaps until we can perfect our knowledge and formulate an adequate plan.

Manifestly, we must learn what kinds of food are necessary; not merely assume that if we supply plenty of hay that the elk will eat it and survive. Of course the winter elk will eat it. Starving animals will eat anything! But we do not know yet that the hay we furnish is the best food we can get or that it is even sufficient to support elk life.

The indications are that it is not sufficient. In the Yellowstone, we had considered that the eating of aspen bark by the elk in winter was an emergency ration, and was only done when the elk were forced to it by hunger. In January, 1920, two hundred of these elk were caught and sent to the Canadian Rocky Mountain Park for restocking purposes. Some of these animals were turned loose and some were sent to other places. When I visited this park in September, 1924, I found about twenty elk in a large enclosure a short distance from Banff, Alberta. Although this enclosure was grassy and large enough to afford considerable forage for the elk, no dependence was placed upon this and the elk were fed regular rations. Still, to my great surprise, I found every one of the aspens in the enclosure had been badly barked and all were dying. In answer to my inquiry, the keeper stated that the elk

had always received ample rations of hay and grain, but that, in addition, they craved aspen bark and insisted upon eating it. So I addressed a letter to Commissioner J. B. Harkin, and, in due time, an answer came back saying that hay and oats, or hay and corn, were fed regularly, but that: "it is realized that rough lands, well watered by streams, having considerable forest area, are better adapted to their wants. They do much better where they have a variety of food, grasses, buds, leaves and twigs." A letter from Dr. W. Reid Blair of the New York Zoological Park, said: "Originally our elk range, comprising some six acres, contained much natural food of elk, but this was soon disposed of by our herd. In the zoological park we must depend on the more or less artificial food. . . . . We are, however, able to cut a small amount of browse from other parts of the park for the elk herd." The assistant director of the National Zoological Park, A. B. Baker, wrote me that oats, bran, cracked corn and mixed hay were fed the elk, but: "When fresh grass and tree trimmings are available, or leaves fall into the inclosure, no hay is given. Freshly cut grass is thrown in and there is a little grazing in the enclosure. The tree trimmings are mostly poplar, maple, willow and birch; the falling leaves mainly poplar and oak. We believe the browse to be good for them."

Even before this evidence began to pile up, I had begun to think that hay and grass, no matter how good, were not enough for the elk, or at least they craved something else in addition. So I determined to find out exactly what the wild elk ate. I commenced to watch them closely. When I had the spot where one was eating carefully marked down, I went directly to it, and was soon able to find the exact plants that the elk had nipped. Usually the fresh plant juices were still trickling out, or the plants were wet with saliva. Soon, I became able to follow the fresh trail of an elk and find the plants it had just eaten. At first thought, it would seem more easy to do this in winter. In some ways, it was easier to track an elk in the snow and find the plant eaten. But the difficulty then was, that the elk usually ate all of every serviceable plant, and did not leave enough for proper identification.

During January, February, and March, I found the elk ate pretty much everything that appeared above the snow, even the stiff, prickly rabbit-bush (*Chrysothamnus graveolens*) and tumbleweed (*Amaranthus albus*). At this time of the year grasses dried and cured on the root seemed to be the main standby, but wild rye grass (*Sitanion montanum*) and the leaves and tops of black sage (*Artemisia tridentata*) were also eaten, although apparently not much relished. But aspen (*Populus tre-*

muloides), cottonwood (*Populus augustifolia* and *Populus acuminata*), and coniferous browse, especially the needles of Douglas fir (*Pseudotsuga mucronata*), were liked better. All branches thrown down in winter by storms, or by other agencies, soon had their twigs and foliage (if evergreen) eaten. Elk even stood on their hind legs to reach and pull down the lowest branches of growing limber pines (*Pinus flexilis*), red cedars (*Juniperus scopulorum*), and cottonwoods. If they succeeded in finding piles, or windrows, of old, fallen aspen or cottonwood leaves, these were greedily eaten. I have seen prickly-pear cactus (*Opuntia polyacantha*) and birch shoots (*Betula fontinalis*) eaten in March in addition to various species of willow shoots (*Salix* sp.).

In April, grass was still an important food but the following were also eaten as soon as the spring growth appeared above the ground: the stems and leaves of balsam-root (*Balsamorrhiza sagittata*); the leaves of strawberries (*Fragaria ovalis glauca* and *Fragaria pauciflora*); the stems and leaves of herbaceous cinquefoil (*Potentilla diversifolia*); and also the leaves of the wild geranium (*Geranium viscosissimum*), if the season was far enough advanced for it to be growing.

In May, grasses ceased to be an important food, although still consumed to a small extent, but their place was taken by the herbaceous cinquefoil that now became extraordinarily common everywhere in the park. In addition, the consumption of geraniums, leaves and stems of balsam-root, strawberry leaves, and leaves and stems of thistles (*Carduus hookerianus* and *Carduus drummondii*) was noticeable.

In June, herbaceous cinquefoil was almost the sole food, but grasses were eaten to a limited extent and also the leaves and blossoms of white clover (*Trifolium repens*). (The latter plants were growing near the roads and buildings where they probably had been unintentionally planted with seeds from bales of foreign hay brought in to feed the stage and cavalry horses that were formerly in the park.)

In July, herbaceous cinquefoil remained the most important food, but the elk also ate lungwort (*Mertensia ciliata*); the leaves, stems and flowers of wild carrots (*Eriogonum* sp.?); the stems and flowers of lupines (*Lupinus pusillus*, *Lupinus caespitosus*, and *Lupinus monticola*); yellow asters (*Aplopappus* sp.?); the leaves of dogtooth violets (*Erythronium parviflorum*); geraniums; the stems and flowers of purple asters (*Aster* sp.? and *Machaeranthera canescens*); sunflower leaves (*Helianthus annuus*, *Helianthus pumilis*, and *Helianthus nuttallii*); the flowers, leaves, and stems of the paintbrush (*Castilleja miniata* and *Castilleja confusa*); and cow parsnips (*Heracleum lanatum*).

In August, herbaceous cinquefoil continued to be the most important food of the elk, but these animals also ate cow parsnips, sedge grasses (*Carex* sp.?), the leaves, stems, and flowers of lupines, the stems and leaves of dwarf sunflowers (*Helianthella quinquenervis*), and grasses to a limited extent.

In September, grasses were eaten a little more, and herbaceous cinquefoil lost its commanding position as a food, possibly because it was now dried up and shriveled. In addition, the elk ate thistle tops, lupine leaves that were now dried and curled, lupine stems and seed pods, dwarf sunflower leaves, geranium leaves, and the dried and curled leaves of the fireweed (*Chamaenerion augustifolium*). While I have not listed the elk's eating of wood parts of shrubs and brush before, it went on to some extent all summer and became more important this month.

In October, the elk ate dwarf sunflower leaves, geranium leaves, lupine leaves and stems and pods, fireweed leaves, tall larkspur (*Delphinium reticulatum* and *Delphinium glaucescens*), cow parsnip, low huckleberry plants (*Vaccinium scoparium*), mushrooms sp.?, porcupine grass (*Stipa spartea*), cone-flower leaves (*Rudbeckia occidentalis*), aspen leaves and twigs, kinnikinick (*Arctostaphylos uva-ursi*), thistle stalks, and the leaves and twigs of shrubby cinquefoil (*Dasiophora fruticosa*). This month the consumption of various grasses was rather greater than at any time during the summer, as was also the taking of various kinds of browse.

In November, the eating of the grasses dried and cured on their roots became greater. At this time the elk also ate the tips of rose bushes (*Rosa sayi*), the tips of gooseberry bushes (*Ribes saxosum* and *Ribes viscosissimum*), fireweed, lupine leaves and tops, tops of tall larkspur, tops and upper leaves of thistles, sedges, willow shoots, old aspen leaves, aspen twigs, dwarf sunflowers, leaves and tips of black sage, white sage (*Eurotia lanata*), dwarf willow (*Salix pseudomyrsinites*), asters, thistle leaves stripped from the stems, and the leaves and stems of cow parsnip.

In December, in mild and open seasons, the elk lived on the same kinds of food as in November. When the weather was cold and stormy, especially if there was much snow on the ground, their food resembled more that eaten in January, February, and March. When the winters ended, I found that there had been heavy browsing on wild rye grass, small aspen trees and red cedars.

Data are now being collected on the exact quantity of each food eaten, and its average rate of growth, but this material is not yet sufficient to

warrant publication. Although the studies already made give us more knowledge of elk food than we have had before, we should persevere at the work until we know much more. After we have found out what the elk eat, we should survey the ranges and see if this preferred food is now growing there.

The elk seem to have ample cover and protection from storms in the Yellowstone region as long as the present forests are undestroyed. But this is because the elk are hardy animals, and mere storms and cold do not hurt the well-fed, healthy ones. On the other hand, sickly animals and very young calves need all the protection they can get.

Since the elk herds have always had natural enemies in the coyotes, wolves, and mountain lions, it is not probable that they cause serious losses. But here again we are confronted by our lack of exact knowledge. We know that coyotes destroy numbers of small calves; that wolves kill some mature animals in winter; and that mountain lions catch many young and sickly animals. Although bears, both black and grizzly, kill a few calves each spring, the number is small.

In the matter of competition for food, we touch upon a subject second only in importance to the elk food question itself. Just how much the smaller animals compete with the elk is another subject upon which we need more study. Nevertheless, I have seen mice, ground squirrels, rabbits, and even porcupines eating the plants needed by elk. We are all sure that the competition from domestic live stock upon the elk range is disastrous, especially in the case of domestic sheep. But even horses, the least destructive of stock, will strip a range of certain important plants.

Recently, a new menace to the elk herds has appeared in the growing "tame herd" of buffalo maintained in the Yellowstone National Park. While this herd was few in numbers, its menace was small, but now that the herd numbers nearly a thousand the competition is being felt by the elk. Already thousands of acres of the Yellowstone area are reserved for the "tame" buffalo herd, and this falls with telling effect upon the elk, because these acres were all former elk winter range. And it is winter range of which we are manifestly short.

In the matter of diseases, the elk no doubt have always had their afflictions, but the white man and his domestic animals have added at least two scourges to the number. Scabies is a domestic sheep disease and it was apparently brought into the Rocky Mountains by the domestic sheep herds. Hemorrhagic septicemia was probably brought to

NEMATODE PARASITES IN THE OVARIES OF THE ANT-EATER (TAMANDUA TETRADACTYLA)

By George B. Wislocki

[*Plate 24*]

In studying the reproductive tracts of members of the Xenarthra, the writer had occasion to observe an unusual form of parasitism in the three-toed ant-eater (*Tamandua*). In two specimens,[1] one obtained in Nicaragua and the other in Guatemala, the ovaries were found to be invaded by nematode parasites. The fact that the ovaries were identical in both instances and that the specimens were obtained at different times in different localities leads one to believe that this form of parasitism may be constant for the genus. No mention has been found in the literature of the occurrence of parasitic worms in the ovaries in either this or any other group of mammals.

The two ant-eaters under consideration were adults. Grossly the ovaries were large and irregular in shape, due to the presence of a variable number of smooth elevations or low protuberances on their surfaces. These tumors could not be accounted for as Graafian follicles because they were solid and although grossly they might have been taken for corpora lutea, this idea was dispelled, because it is the rule for the ant-eaters to produce but a single young at a time.

On my examining sections of the tissue it became immediately evident that the protuberances are due to the presence of nematode worms situated in the ovaries. The parasites occur to the number of from two to ten in a single ovary. Each parasite lies closely coiled in the stroma of the ovary, some near the surface, others in the interior of the organ (fig. 1). About the parasites there is a pronounced connective tissue induration and infiltration of free cells (fig. 2). Eosinophile cells constitute a large fraction of the accumulated cells.

The ovarian tissue is easily recognizable in the interspaces between the parasites. There are numerous primary follicles, some small normal Graafian follicles and a large number of atretic follicles (fig. 1). The latter are of considerable interest, showing all degrees of transition between follicles in which there is a beginning proliferation of the theca interna, to follicles in which there is just a remnant of granulosa left in

[1] The specimen from Guatemala was obtained from the U. S. National Museum through the courtesy of Gerrit S. Miller, Jr., and A. Brazier Howell.

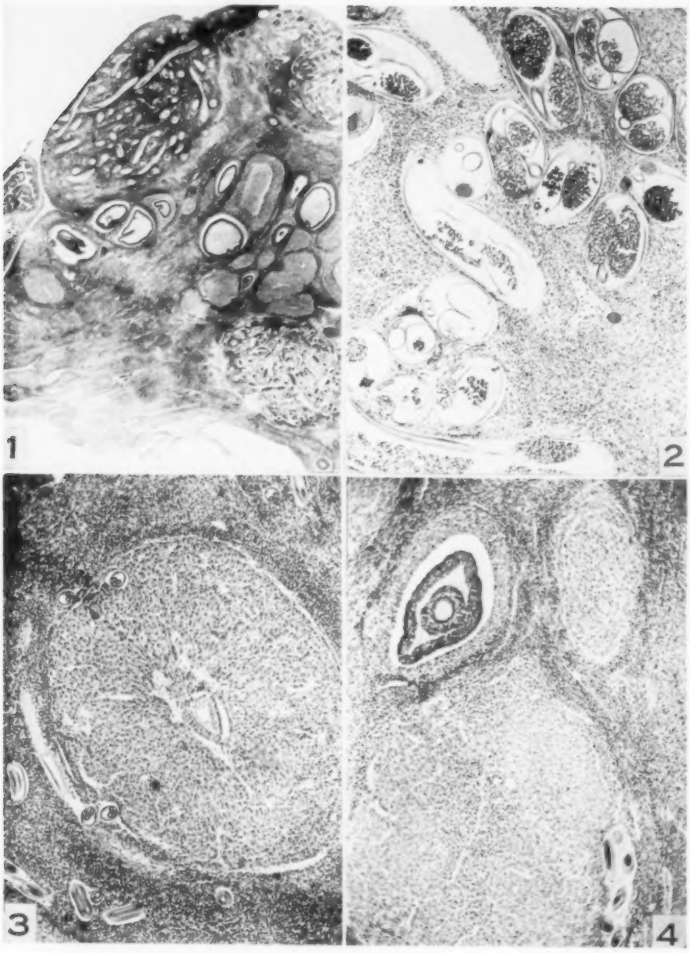

FIG. 1. Cross section of the ovary of *Tamandua tetradactyla*, showing parasitic nematodes imbedded in the stroma of the ovaries. Besides the parasites, there are numerous Graafian follicles, most of which are atretic. To the extreme left a portion of the Fallopian tube. × 8.

FIG. 2. Coils of a parasite located in the ovarian stroma, showing a marked local infiltration of cells. × 40.

FIG. 3. An atretic follicle composed of proliferated thecal cells with a barely perceptible remnant of the granulosa in the center. At the edge of the follicle and in the surrounding stroma numerous young parasites. × 40.

FIG. 4. Another group of atretic follicles. In the upper left hand corner a Graafian follicle with a well defined granulosa, surrounded by a layer of thecal cells beginning to proliferate. × 40.

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the center of the atretic follicle (figs. 3 and 4). Finally there are a few follicles in which all trace of the granulosa has disappeared, leaving a mass of proliferated theca interna cells which resembles very closely a small corpus luteum. It is apparent, however, that these are not true corpora lutea but only the end-stages of the process of follicular atresia which can be followed in all its phases. As in the rabbit's ovary the atretic follicles retain for a long period their circumscribed character, instead of breaking up and blending with the interstitial tissue as happens in the majority of mammalian ovaries. Interspersed amongst the atretic follicles, occupying the tissue spaces and lymph-channels of the stroma of the ovary, immature stages of the parasites can be seen (fig. 3).

The nematode parasites under discussion belong to the group of filarial worms[2] a great number of which are parasitic for animals. A form localized in the ovary has never been observed before. Examination of the tissues of the hosts reveals no other pathology than that described in the ovaries.

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[2] A study of the parasites is being made by Dr. G. Steiner, of the U. S. Department of Agriculture.

BEAVERS IN THE ELK MOUNTAIN REGION, COLORADO

By Edward R. Warren

In July, 1927, I visited part of the Elk Mountain region, Gunnison County, Colorado, and paid particular attention to the beaver colonies there. A good part of my time was spent at Crested Butte, where some twenty-five years ago I made rather extended observations on the beaver workings on Slate River. A great increase in the number of beavers was evident everywhere I went. The continual close season has had its effect, even though some poaching may be going on. I also visited Brush Creek, to the eastward of Crested Butte, Lake Brennan at Irwin, and was on Anthracite Creek to the westward, and noted indications of beavers everywhere. In fact I found them where I had never known them to be during my residence in the region twenty-five years and more previously. In the following remarks I shall take up the Slate River Valley first, as I have already published a paper on this area based on observations made in 1902 and earlier, and include some notes made in 1905 and 1910.

The Slate River Valley (fig. 1) is above the town of Crested Butte. The stream rises in the high mountains about ten miles northwesterly from the town, and flows eastward past that place. The valley where the studies were made is about 3½ miles long, and the lower two-thirds about half a mile wide. The whole region under consideration lies between the 8900 and 9000 foot contours. The width of the stream varies from twenty to forty feet, and more at times, and after the high-water season in early summer has passed, its depth varies from a few inches to four feet and more in the deep holes. This valley is really a large beaver meadow, overgrown with two species of willows (*Salix planifolia monica* and *S. geyeriana*), and long, coarse swamp or slough grass. Through this the stream pursues a sinuous course, with many oxbow bends. The river has cut across many of these and left the abandoned portions as sloughs. All over the area are channels or sloughs with little or no current in them. There were too many of these to be examined in the brief time at my disposal. One of the principal of the smaller channels is on the easterly side of the valley. In the summer season the willows, which are from three to ten feet high, grow so thickly that it is difficult for anyone to get about away from the river, and the foliage is so dense that it is almost impossible to discover

anything at this time unless one happens to be close to the place. Autumn, after the leaves have fallen, is the ideal time to work in these places. In October, 1905, I made a second study, though not a very detailed one, of the area, and also in October, 1910, I gave part of a day to the place. In July, 1927, I spent a good part of five days on this ground, and made a quite thorough examination of the work along the

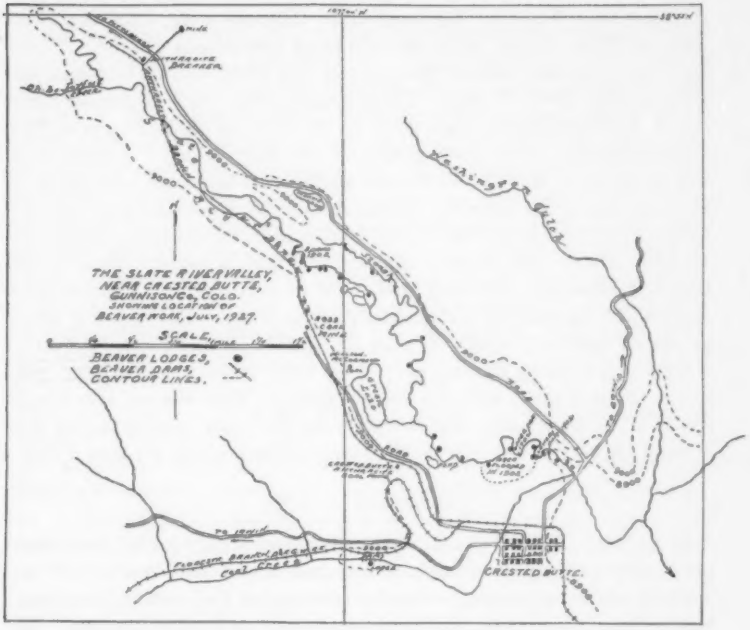

FIG. 1

river though I was somewhat handicapped by the thick brush and high water.

In the Proceedings of the Washington Academy of Sciences (vol. 6, pp. 429–437, February 28, 1905) I published an article entitled "Some Interesting Beaver Dams in Colorado," which dealt with the beaver work on Slate River. The paper was based on observations made in 1902 and the three preceding years. The maps were drawn from a

survey made in November, 1902, which fortunately happened to be an unusually mild and open month for that country, with no snow to speak of until the latter part of the month. As a contrast, in October, 1905, there were snowfalls of several inches each, and snow was on the ground when I made my examinations that year, while the mercury went down nearly to zero several times.

A brief summary of the above mentioned paper may aid in understanding what follows. The area dealt with was along more than two miles of Slate River, with the adjoining overflowed ground. There were two principal beaver dams across the river, one 76 feet and the other nearly 100 feet long. These formed two ponds of considerable size, each containing a lodge, while to the south of the river, among the various sloughs, were more dams and one large house. A large area was overflowed, but the water was shallow. There was much sign of activity among the beavers. Along the stream above the pond formed by the upper of the two dams were trails and willow cuttings, and about 1½ miles above were sloughs and a pond with a lodge. Then about three-quarters of a mile still farther up were two series of three dams each, which were somewhat particularly described. The upper of these groups of dams was shown in plate 32, figure 1 in that paper (loc. cit.). Abandoned work on the east side of the river valley was also described, and a drained pond with the lodge figured. This was on the slough south of Nichols Lake, and is shown on the map accompanying the present paper, but was outside the limits of the maps in my first article.

CONDITIONS IN 1905

In October, 1905, I found that the lowest dam of 1902 had been practically destroyed by high water, only about twenty-five feet remaining at the right bank. The second or upper dam had been entirely washed away, and then rebuilt at practically its former location. On the tenth, when I first saw it, there was a break or gap in it. This had been entirely closed by the following morning. There was also another dam above this one. I could see evidence that there were many beavers on the stream. There were many small dams across the sloughs, and the banks of the latter had been built up to keep them from overflowing. Most of this work appeared to be fresh. The water seemed to be deeper over this part of the valley than in 1902. The slough grass was very tall and there were many well beaten trails through it.

The beaver signs did not seem to be as plenty as in 1902 for some distance above the second dam of that year; then there was much evidence of the animals, such as sticks in the water and trails and slides on the banks. I was informed that the group of three dams had been washed out by the high water, and had been rebuilt in 1905, and that only shortly before my visit had the beavers ceased work on them. My informant was a man who rode by the place daily on his way to work.

Between October 11 and 19 several inches of snow fell, much of which was still on the ground when I made my last visit. I found many trails in the snow among the willows. The trodden portions of these trails were well packed, and on one side the snow was scratched and rubbed by the brush that the beavers had been dragging. They seemed to have always had the loose ends on the right side, so that the beaten path was to the left-hand side of the trail going toward the water or house, and the scratched part on the right. The total width of the trail was two feet or more. Many of these trails had been used the night before I saw them. (At the Cooke City road, near Camp Roosevelt in Yellowstone National Park, the beavers dragged the brush at their left sides, but that was probably because that was the lower side as they came diagonally down the hillside with their burdens of rose bushes or small aspens.)

CONDITIONS IN 1910

On October 7, 1910, I found that the two long dams of 1902 had entirely disappeared. I also found various new dams in the sloughs, built since 1905, besides a lodge. The large house mentioned above was still there, but a lodge in a pond some distance above, which I had known of since 1900, had disappeared, and the pond was much overgrown with grass. All along the river I found plenty of beaver signs.

CONDITIONS IN 1927

In my 1927 investigations I covered all that part of Slate River above the road leading out of Crested Butte on the northeast to about a quarter of a mile above the second bridge where the railroad crosses the stream. I was also on the easterly side of the valley at the slough or channel there. From the observations made on the several days given to the work I think it is safe to say that the valley is supporting a very large beaver population. As to how many individuals I do not

hazard a guess. Along the river itself I found twenty-two lodges, and one at the slough on the east side. With but one or two exceptions all of these lodges appeared to be occupied. One lodge had a water-worn piece of two-by-four on top, another had many pine sticks upon it, and a third had an old railroad tie lying on it. These lodges were all built on the bank, usually close to the water, the most distant being thirty feet away. About two-thirds of the houses were on the right bank. An interesting thing in connection with this can be seen on an inspection of the map. Along that part of the stream having a general easterly course, though with many bends, it will be noticed that there are a succession of lodges on the right bank, where the river is nearest the road on the northeast side of the valley. Then above, where the road on the westerly side is the nearer, the houses are on the left bank. Whether these roads had anything to do with this it is of course impossible to say. I would remark, however, that it was to the south of the part of the stream where the lodges are on the right bank that a large area was flooded in 1902.

Besides the lodges there were a great number of trails leading from the water, and much willow brush and peeled sticks in the water and on the shore. Often one saw considerable accumulations of beaver-cut brush stranded on gravel bars in the stream, and occasionally I found the remains of a winter food store in the water near a lodge. Although the banks of the river are rather low and flat, it is possible that a certain number of the beavers were living in burrows. I found a number of chambers or cavities in the banks, just below the surface of the ground, and large enough for one beaver, or at most two. These chambers were connected with the stream by underground passages. Two that I found seven feet apart were twenty feet from the water, and each had its individual entrance. One chamber was 22 by 42 inches, 9 inches deep inside, with the bottom 15 inches below the surface. The other was 18 by 30 inches, and 9 deep inside; the bottom 18 inches below the surface. The first was now entirely uncovered, a few old sticks lying about; the other was fairly well covered with old sticks, broken through in two or three places. One or two of these chambers seen elsewhere were pretty well covered, and had had mud mixed with the sticks. Doubtless this was the original condition with all of them. The two above described were the farthest away from the shore that I found; others were much closer, one being but three feet from the water. One can only speculate as to why single beavers made

and lived in these places, if such was the case. Perhaps they were an overflow from the lodges. The form and situation of these chambers suggest that they are the first forms of a bank lodge, or of a lodge of any type.

Another thing which I noticed on Slate River was the piles of mud I found here and there on the banks. I found as many as six close together. Some of them were a foot high and eighteen inches in diameter at the base. They may have been scented with castoreum, but I detected no odor at the few where I investigated this point. They were evidently made for some purpose, as their form was too regular for mere piles of mud thrown out by the beavers in digging burrows, even if such were likely to be thrown up on the bank.

At one place a wire fence with aspen posts was beside the river. The beavers had shown their preference for aspen bark by gnawing these posts. One post had a notch cut in it six feet above the ground, besides another near the bottom. The former must have been made when the snow was deep. The dense growth of willows in this valley would seem to assure a supply of food for a large number of beavers for a long time to come.

The skinned carcass of a beaver was found lodged against a driftwood pile near the place where the three dams were in 1902.

Henry Spencer, the forest ranger of this district, informs me that there are beavers along Slate River as far up as the old mining camp of Pittsburgh, and in Poverty Gluch a mile above that place. In 1900 there were large deserted ponds along the river a mile below the camp, and a little fresh work.

He also tells me that on East River, above the old camp of Gothic, a wide meadow is well stocked with beavers, which are on the stream as far up as there is any place for them, and that they are on the smaller side streams. The three Brush Creeks are well stocked with beavers, which are causing trouble by overflowing trails. A fence was built on Brush Creek of aspen logs which the beavers had cut. In general the whole of this part of the Elk Mountain region is becoming well stocked with beavers.

Coal Creek

Coal Creek is a tributary of Slate River the head-waters of which are north of the deserted town of Irwin, and which finally flows easterly, passing diagonally across the town of Crested Butte. It is a fair-sized stream with a good flow of water. During the years when I lived in

the region I never saw any signs of beavers in the creek. In October, 1910, I noticed a dam across the stream not far above its mouth. I had been informed by a correspondent before I went to the place in 1927 that there were beavers in Coal Creek, and this proved to be the case. Only about half a mile above the town I found four deserted dams and ponds, as well as a lodge. A colony had been there not long ago.

On July 7 I found a group of dams about 3½ miles west of the town, where the valley widened somewhat. Here were several ponds. The lowest dam, about fifty feet long, had been cut through by high water, which had made a gap about eight feet wide. I found the dam where broken to be 5½ feet high from the stream bottom to the top. The pond had been three feet deep near the left end of the dam, away from the channel. Farther upstream were three more dams of greater length, as well as smaller ones across small side channels. In constructing one of the long dams the beavers utilized a log jam by building up against it and out from its left end to the bank. I called this the "Driftwood Dam." About these ponds I found used trails and freshly cut willows, signifying that the place was occupied.

Lake Brennan

Lake Brennan is an attractive body of water extending northwesterly from the Irwin townsite for about one-third of a mile, and having a width of perhaps two hundred yards. One looks over it from the southeasterly end directly toward the Ruby Peaks, the summits of which are less than two miles distant. About most of the shoreline are scattering Engelmann spruces, and at what one may call the north corner, where Anthracite Creek enters the lake, is a meadow densely covered with willows (*Salix planifolia monica*). Anthracite Creek flows from the lake near the northwest end. In this willow-covered meadow a colony of beavers was living, with two lodges, one close to the lake shore, 16 by 18 feet on the ground, and 5½ feet high above water level, with water three feet deep beside it in the lake.

About 75 feet above the mouth of the inlet was a dam some fifty feet long, a hundred feet above that another a hundred feet long, and 150 feet above this a third dam 240 feet long. This last was one of the most crooked dams I have ever seen. So crooked that I had the curiosity to make a survey to find out just how crooked it was. The accompanying sketch (fig. 2) shows this. In the upper part of the pond formed by this dam was a lodge, having, I think, the most pronounced conical shape of any lodge I ever saw. It was twelve feet in diameter

at the ground level and six feet high. The water was two feet deep beside it. The surrounding willows made it difficult to procure a photograph that showed the shape of this house satisfactorily. This whole area was cut up by many small channels on which were various small dams to control the water, and the whole place appeared to be flooded. Many of the willows had been cut at a height that indicated that it was done when the snow was still quite deep. At that altitude, 10,000 feet, the snowfall is heavy and spring is late. The beavers seemed to confine their activities to the above-described willow-covered area, and there were no signs of them along the shores except for a short distance on the northeast side.

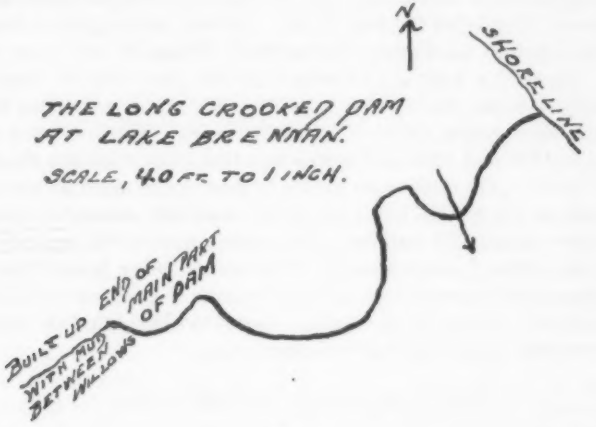
N
SHORE LINE
THE LONG CROOKED DAM
AT LAKE BRENNAN.
SCALE, 40 FT. TO 1 INCH.
BUILT UP WITH MUD BETWEEN WILLOWS
END OF MAIN PART OF DAM

FIG. 2

We had an interesting experience watching a beaver swimming in the lake. My assistant was standing by the first-mentioned house, while I was a short distance away preparing to take a picture, when we saw a beaver swimming about near the shore. This was at about nine in the morning. We watched it some ten minutes, and then I took my picture and went ashore, tired of standing nearly to my knees in cold water. I stood and watched the animal some twenty minutes longer. It swam about without paying any attention to either of us, sometimes close to the shore, then again well out in the lake. It came near enough to me once or twice so that I could form some idea of its size and judged

it to be a yearling. After it had been swimming around a short time it began diving with slaps of its tail on the water, coming up immediately and repeating very soon after. We went on above to examine the other work, returning in about an hour, and the beaver was still enjoying its morning exercise and fun, for we considered this tail-slapping as nothing more than play. During all the time we had been about, this had been going on. It could be heard, and occasionally I was high enough on a hill-side to be able to see it when some distance from the place. Altogether this beaver was out in mid-morning more than one and one-half hours, apparently doing nothing more than swimming for pleasure.

The presence of beavers in Lake Brennan is a comparatively recent occurrence, that is for the present day. I know nothing about them in the times before the country was settled. Probably they were there then. In 1879 a mining excitement started, the town of Irwin was established, many mining claims located, and some real mines found, and the whole region pitted with prospect holes. The boom was at its height in 1880 and 1881, and it was said that there were five thousand people there. The excitement gradually died down, aided by the fall in the price of silver, until but a few people were left, connected with the one or two mines still working. Prospectors were in the region before 1879, and while I knew some of these men I never heard them say anything about beavers here, perhaps because it never occurred to me to ask them. In any event I think it more than likely that the animals have returned within the last ten years or so.

Beaver Colonies near Anthracite Creek

Anthracite Creek not only supplies Lake Brennan with water but is also its outlet, leaving the lake near the northerly end of the southwest side. It flows in a more or less northwesterly course, joined here and there by various tributaries, until it unites with Muddy Creek coming in from the north, the two streams forming the North Fork of the Gunnison. The Ute Indians had a trail along the stream on which they traveled to-and-fro between the White River and the Gunnison. In later days white men used it. Within a few years a good road has been constructed, which follows the general route of the trail for some distance. Leaving Lake Brennan and crossing Kebler Pass, we drove down this road to the point where it crossed the stream at the same place as where the old train had crossed and made our camp a little beyond, in the midst of a wild flower garden. I counted more than twenty species of

flowers within a short distance of the tent. The altitude here was about 8800 feet.

When driving down the road we could see beaver work in the stream, and on a hillside opposite our camp could be seen freshly felled aspens, while there was evidence of beavers in the stream here.

I spent my first afternoon at this camp fishing above the road crossing, but that did not prevent my noticing the beaver work, which was everywhere. I do not know what distance on the creek I traversed. These streams are too crooked for one to guess at that, though after I left it I reached the road a half-mile or more above the bridge, and it is quite possible that I had followed a mile of the stream. There were several dams which had been broken by high water, some of which had once formed good-sized ponds. Many willow cuttings were lying in the water and on the banks; trails, landing places, and tracks were common. I saw one very large house. There were many large alders growing beside the water and the beavers had cut a good number of these. I hoped to make a more thorough examination of the work along this creek, but gave so much time to the three colonies described below that this was not possible.

These three colonies were hidden away in hollows among the hills, so that it was a surprise when one came upon any of them, though a traveled trail passed them all. They were situated northeasterly from our camp, on the northerly side of the creek, on the lowermost slopes or foothills of the Ruby Range. My information about them came from H. L. Curtiss of Paonia, who had visited them a short time before. I did not anticipate giving much time to them, but found them so interesting that practically all my working time on Anthracite Creek was spent there, and my companion, Dobson West, made rough surveys from which sketch maps were drawn. With the aid of my small aneroid I made the elevation of the lower colony to be 8815 feet, that of the middle colony 8840 feet, and of the uppermost 9075 feet.

The Lower Colony. The lower colony consisted of one large pond of irregular shape, and two more below it, the second being partly full of water, while the third was empty. The large pond covered an area of about three-fourths of an acre, and contained one lodge well out in the water away from the shore. This looked rather old. In the southeasterly corner of the pond was a little island with old aspen stumps on it. Evidently this was once a knoll that had been made into an island by the flooding of the hollow. The slopes all about the pond were covered with aspens, which began about a hundred feet from the water,

and there were trails all around the shores leading up to these trees. The beavers seemed to be cutting them everywhere. Two weeks before my visit Curtiss saw eight beavers in the water at one time at midday. The aspens evidently once grew nearer the water, as the old stumps showed. There was no inlet to this pond, so presumably it received its water supply from springs now out of sight in the bottom of the pond. On the hillside above the north end of the pond were two or three small springy places, but no surface water appeared to reach the pond from these.

The dam forming the pond was rather crooked, 174 feet long, and 5½ feet high on the face. Another dam was 260 feet below this, the pond to which was not quite full of water. Nine feet from the northerly end of this dam was a house, built on the dam. It was 22 feet along the dam by 12 feet the other way, 5 feet high above the dam and 8 feet above the bottom of the pond below. A little spring run entered the middle pond from the north side hill. The lowermost dam was a tremendous structure. There was a gap 14 feet wide in it, how or why made I do not know. The dam was 7½ feet high above the ground level in the gap, and 21 feet across the base. Its total length was 246 feet. The top was 8½ feet above the pond level above.

The Middle Colony. The divide or ridge between the lower and middle colonies had an elevation of 8900 feet, 85 feet higher than the former and 60 feet above the latter. This group of ponds lay in a hollow or basin bounded on the south and the east by a rather steep hillside, part of which was barren slide rock, and part had a good stand of aspens. The land northerly from this was comparatively level for some 300 to 500 feet, then the ground sloped up once more. At the east side was the inlet furnishing the water supply, a rivulet a few inches wide flowing down the hill. There were two principal ponds in the group, and three or four smaller ones. The first pond had a dam 130 feet long, but was narrow, less than a hundred feet wide. The little stream entered this. The water in this pond was muddy when it was seen. It contained many alders killed by flooding. Below it were a couple of smaller ponds; then came the largest and principal pond of the colony, one shore of which was formed by the steep base of the hill, while the opposite shore was extremely sinuous and irregular in outline, due to the flat surface of the ground bordering it. On the north bank at the east end of this pond was a lodge, which was five feet high above the water level and four above the ground level. The water was two feet deep a little farther away. This lodge was approximately sixteen feet in

diameter. On the other side of the pond, almost opposite this lodge, was another of good size and fifty or more feet away from the bank. Along the easterly side of the pond were a number of short canals. This shore had a scattering growth of willows, which became thicker toward the north end, and this end of the pond was full of willow bushes. At one place on the shore was a little grove of aspens, mostly good-sized trees up to twelve inches through. Several of these had been felled and one log a foot in diameter had been completely stripped

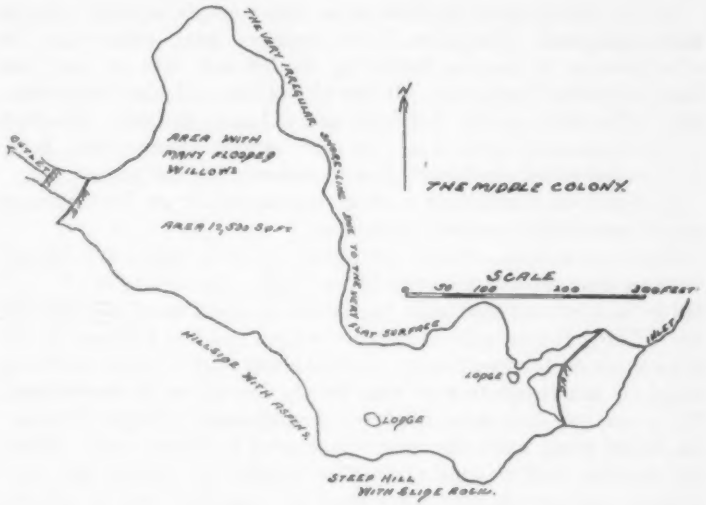

FIG. 3

of its bark. I saw none of the rough outer bark lying about. A number of these large standing trees had notches cut into them. Other felled aspens were noted that had also been denuded of their bark.

The dam that formed this pond was a very short one to have made a pond more than six hundred feet long, being only seventy-five feet in length. The face was five feet high above the water below, which was thirty inches deep. The dam was a very wide one, as was easily seen, the water in the pond being three feet below the crest of the dam, the distance across from the crest to the water being fourteen feet.

2. Descriptions are given of the changes in beaver work on Slate River as compared with 1902, 1905, and 1910. Very great changes had taken place, the number of beavers had apparently greatly increased, and there was an abundance of willows for food.

3. Beaver colonies were found on Coal Creek, where there was none twenty-five years before.

4. A colony was found at the north end of Lake Brennan, Irwin, where there had been none for many years, at least.

5. At Lake Brennan a beaver was seen swimming in daytime, diving and slapping the water with its tail, apparently in sport, certainly not because it was alarmed.

6. Much beaver work was noticed on Anthracite Creek, and the animals had increased greatly in numbers of late years.

7. Three interesting colonies in the foothills near Anthracite Creek were studied. These all received their water supply from springs or very small streams. The food supply here was mostly aspens.

8. Various streams in the region had much beaver work on them, and all reports showed that the continued close season on beavers in Colorado had been very beneficial in increasing the number of beavers here.

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GENERAL NOTES

A LOCALITY RECORD FOR SOREX ORNATUS

The collecting of an adult male *Sorex ornatus ornatus* at Adobe Falls in San Diego County, California, within ten miles of the sea and at an elevation not exceeding 500 feet, is of interest. Adobe Falls is in a branch of Mission Valley, about three miles from San Diego. This shrew, no. 39 in my collection, was taken by hand at about 3 p.m., March 5, 1927. It was jumping along through the grass when first seen. Identification was made by Dr. H. H. T. Jackson.—FRANK F. GANDER, *East San Diego, California.*

CRYPTOTIS PARVA IN CENTRAL NEW YORK

On November 1, 1927, a small short-tailed shrew (*Cryptotis parva*), which had been found dead on the campus of Cornell University, was brought to me. In making an examination of the mammal collection in the University Museum, I found another Ithaca specimen of this shrew. Both are adult males, and the average measurements are: total length, 72 mm.; tail, 14.5 mm.; hind foot, 11 mm. These two New York specimens average considerably smaller than three in the university collection that were collected in Georgia. It is interesting to note

that the two, while taken three years apart, were collected within 200 yards of one another.

Miller (North Amer. Recent Mammals 1923, 1924) records the range of *Cryptotis parva* as the Austral region of the eastern United States (including both the Austroriparian and Carolinian faunas) from Texas and eastern Nebraska eastward to the Atlantic coast from Staten Island southward. The only previously recorded shrew of this genus from New York is a Staten Island specimen in the collection of the American Museum of Natural History.

This materially extends the known range of *Cryptotis* in New York. It is probably not as scarce as is commonly supposed, and further trapping in half-cleared fields will likely bring others to light.—W. J. Hamilton, Jr., *Cornell University, Ithaca, New York.*

HOW FAST DOES A BLACK BEAR CLIMB?

Recently (June 1, 1927), while I was riding along one of the little-used trails near the north boundary of the Yellowstone Park, my horse snorted loudly and, planting his front feet firmly together in the trail, came to a dead stop.

Glancing down the steep hill, along which we were traveling, I saw, about fifty yards away, what had surprised my mount. An immense black bear was standing on his hind legs and reaching up his full length on a fir tree. He immediately came down on all fours on hearing the horse snort and looked carefully all around to see what had made the noise. He even walked a step or two to look behind the tree. For a full minute he peered intently in all directions, but as both my horse and myself remained perfectly quiet the bear did not see us, and stepped back to the tree.

He looked up into the tree again, then all around once more. Apparently satisfied, he reared himself on his hind legs and started to scratch on the rough bark, evidently seeking a firm hold to begin the ascent. Having found just the grip he wanted, he gave a mighty spring and scrambled up the tree to a height of thirty or thirty-five feet with an astonishing rapidity that I do not believe a pine squirrel could have equaled.

Having attained his objective, which was some large limbs of the tree, he straddled one limb with his hind legs and, with his front paws resting on another limb, lay his shaggy head down on his paws and promptly fell asleep. Water was dripping from his fur, which indicated that he had just come from a bath in a nearby mud wallow.

My horse was apparently as intensely interested as I and did not move a muscle during the performance. When I was certain that the bear was asleep, I carefully dismounted and started down the hill for a closer view. For the first twenty-five yards I crept along very carefully, making hardly a sound, then in some awkward way I dislodged a small rock and down the slope it went, making a horrible clatter.

Bruin was awake at the first sound and watched the progress of the rock down the hill very intently. I, of course, stood absolutely still and waited so until the bear laid his head down again, which was perhaps in a minute or two, although it seemed five or ten minutes.

The old fellow did not go to sleep again, however, but was lying there watching to find out what was disturbing his rest. At my first slight movement he raised his head again, wide awake and angry when he saw that a man was near. Quickly

getting to his feet on the limbs he made as if to start down and I, not wanting to race a big black bear that had just shown me his pine squirrel speed, withdrew from the field.

My horse probably marveled at the agility I demonstrated in getting back up the hill and into the saddle.—W. M. Rush, *U. S. Forest Service, Livingston, Montana.*

MELANISM AMONG THE MARMOTS OF THE TETON RANGE, WYOMING

During the past summer (1927) I noted among the marmots of the Teton Range in northwestern Wyoming four cases of what I interpreted to be melanism. The animals observed were no doubt *Marmota flaviventris nosophora* Howell, the golden-mantled marmot, for the Teton Range lies clearly within the range of this animal, as indicated by Howell[1] and Cary.[2] In the literature descriptive of this marmot I have been able to find no reference to melanism, nor have any instances previously come to my attention though I have spent several summers in different areas within the range of the animal. All four cases were encountered on the eastern slope of the Teton Range in the twenty-mile section extending from Teton Pass northward to the Grand Teton.[3]

The first melanistic marmot I discovered was observed on three different occasions near its den, which was located on a rock outcrop three miles north of Teton Pass at an altitude of about 7000 feet. Another marmot of normal coloration had a den a few hundred yards away. The last time I was in the vicinity I managed to approach to within fifteen feet of the melanistic marmot without its taking alarm. In habits and proportions it did not differ from its neighbor and the numerous other marmots found in the region; but in coloration it was strikingly unlike any I had ever seen.

As observed at this close range the animal appeared entirely black save for a sprinkling of lighter hairs on its forehead, giving the effect of a dark gray spot between the eyes. So far as I could determine these lighter hairs were entirely absent over the rest of the body. Finally the marmot dropped down into a rock crevice from the top of which I caught a momentary glimpse of it at much closer range, possibly ten feet, before it disappeared into one of its holes. I then was able to note what before had not been evident: that the back, like the front parts, was perfectly black, but that the end of the tail over a length of about two inches was reddish brown and quite free from black hairs. The contrast between this brown tip and the coal-black body was unusual and striking.

The other three instances of melanism were all discovered on the slopes of the Grand Teton, highest peak in the range, at considerable distance from each other. One was below timberline, at about 8500 feet; one at timberline, 10,500 feet; and

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[1] *Revision of the American Marmots*, by Arthur H. Howell. North Amer. Fauna no. 37. Pp. 38, 46–49.

[2] *Life Zone Investigations in Wyoming*, by Merritt Cary. North Amer. Fauna no. 42. Pp. 43, 48, and 51.

[3] See map of Teton National Forest, published by the Forest Service, U. S. Department of Agriculture, or the Grand Teton Quadrangle, a topographic map published by the U. S. Geological Survey.

one well above timberline, at about 11,000 feet. The one at timberline was seen near another marmot of ordinary coloration; the other two were solitary at the time observed. In none of the three cases was I able to get closer than perhaps one hundred yards. All three animals appeared to be an even black in color, like the one earlier seen north of Teton Pass. Grey or brown spots might have been present, in the pelage of these animals, as in that of the first, but if so they escaped detection due either to the distance or to the fact that the marmots were part of the time running.

In the course of an attempted ascent of the Grand Teton made early in August, 1926, John D. Haines of Hominy, Oklahoma, and I caught a fleeting glimpse of a small black animal a couple of hundred feet below the so-called "second saddle" of the peak, at an altitude of about 12,500 feet. It disappeared under the rocks so quickly that we did not have opportunity to observe it long or closely, and its identity remained a mystery, neither of us having ever seen any animal like it in the mountains. The animal was about the size of a marmot but so different in coloration that at the time it did not seem likely to me that it could be one. Now, however, I strongly incline to the belief that such was the case.

Several trappers and hunters of long experience in Jackson Hole (the mountain basin at the eastern base of the Tetons and lying between this range and the Gros Ventre Mountains) whom I quizzed regarding these interesting animals at once stated that they had occasionally encountered black "rockchucks," as the marmots are commonly called there; and two of them asserted that the chances of discovering black individuals were best in the vicinity of Teton Pass. It may be that this color phase is a purely local affair occurring in mountain-bound Jackson Hole. Marmots of ordinary coloration are here very abundant, and it would appear that melanistic individuals are not of extreme rarity.—F. M. Fryxell, *Dept. of Geology, University of Chicago, Chicago, Illinois.*

IDENTITY OF THE NORTHWEST COLORADO POCKET MOUSE

In 1906, Merritt Cary, while in pursuance of his "Biological Survey of Colorado" (North Amer. Fauna no. 33, 1911), discovered unmistakable evidence of pocket mice near Snake River, Routt (now Moffat) County, Colorado. He was unable at that time to procure specimens, but on geographic grounds provisionally referred this evidence to *Perognathus callistus* Osgood, a species described from the Kinney Ranch, Sweetwater County, Wyoming.

A year or two later, Edward R. Warren collected in the same locality and, like Cary, noted evidence of pocket mice, but obtained no specimens. He followed Cary's course in tentatively including *callistus* in his list (Mammals of Colorado, 1910).

May 4 and 5, 1924, I procured two pocket mice near Snake River, about seven miles above its juncture with Bear River; and at Two Bar Spring, twenty miles west, eight more were taken, May 11 to 23, 1924. These specimens have been examined by Arthur H. Howell of the U. S. Biological Survey, who reports that they agree well with *callistus*, though the skulls average slightly larger.—Frederic W. Miller, *Colorado Museum, Denver, Colorado.*

THE RANGE OF REITHRODONTOMYS ALBESCENS ALBESCENS

The last published information relative to the range of this mouse is given by Miller (1924, List of North American Recent Mammals 1923), in which the data supplied by Howell (1914, Revision of the American Harvest Mice, N. A. Fauna no. 36) is followed. The range, as outlined in these publications, comprises for the most part the sandhill region of western Nebraska, adjacent parts of South Dakota, and extends southwesterly to Loveland, in northeastern Colorado.

A series of harvest mice, collected during 1921 and 1922 in the extreme southeastern section of Colorado, has been submitted to the U. S. Biological Survey for examination. These specimens have been identified by A. H. Howell as *Reithrodontomys albescens albescens* Cary. This identification materially extends the range of the race.

The mice were collected at Sand Canyon, western Baca County; at Furnace Canyon, eastern Las Animas County; and in the area locally known as "Three Corners," the junction of the boundaries of Oklahoma, New Mexico, and Colorado. Zonally these localities are all in the arid piñon and cedar belt of the lower portion of the Upper Sonoran zone.—FREDERIC W. MILLER, *Colorado Museum of Natural History, Denver, Colorado.*

RECENT LITERATURE

De Laguna, Grace A. SPEECH: ITS FUNCTION AND DEVELOPMENT. New Haven: Yale University Press. 8vo, pp. xii + 363. Cloth, $5.00. 1927.

This is the best book on the origin of language. Modern, original, well written, it is an important contribution to psychological theory and to the study of animal behavior. Professor de Laguna shows that the purpose of primitive speech could not have been to convey information. The use of an animal's cry is not to "convey ideas," but to influence the behavior of some other animal. The earliest human speech was only a modified animal cry, and its purpose, similarly, was to influence the behavior of some other human being—a growl or a roar to scare away an enemy, a scream to summon companions to aid in a fight, a soft love call to attract the beloved one. The authoress makes a penetrating analysis of the difference between articulate language and the animal cry. For example, a hound, upon discovering a fresh trail, starts running along it with loud baying, and other hounds, hearing him, are drawn to the spot. But the baying of the first hound expresses only his excitement, and it arouses only a similar excitement and vague expectation in the minds of the other hounds. One hound can not inform another as to the species of the quarry he has started, or the direction in which it is fleeing. Indeed, the animal can perceive an object only in its relation to his own impulses and actions. Therefore he has no "information" concerning it. In order to have information about an object he would need to perceive it in its relation to other objects independent of himself. Inability to do this was well illustrated by Koehler's chimpanzees. When a banana was suspended too high for any ape to reach it, each chimpanzee was clever enough to bring a box to climb up on; he might even bring a second box, place it on the first, and

climb up the two boxes to the banana. He could see the boxes as something-to-be-climbed-on. He could not clearly see the two boxes in their relation to each other, as was proved by his repeatedly placing the second box on the first in such a position that it fell off. And he could not convey information regarding the boxes, as was proved by the fact that the chimpanzees showed no true coöperation even when they were all dragging the same boxes in order to reach the same banana. From this stage to the human, the evolution of thought depended upon the simultaneous development of articulate language and the use of tools. In experimenting with tools, early man learned to perceive the relation of one object to another, and the effect of one object on another—not merely its effect upon his own feelings. And whereas at first his cries expressed only his feelings, now his cries came to represent relations between objects. Thus they became articulate speech.—WALLACE CRAIG.

Eifrig, C. W. G. OUR GREAT OUTDOORS: MAMMALS. Chicago: Rand McNally and Company. 8vo, pp. xiii + 257, text figs. 177, 2 col. pls. Cloth, $1.25. July, 1928.

We have here the third popular general book on our mammals that has appeared within a year, yet withal there is essentially no duplication. The present volume fills a useful niche in our literature in that, while it deals more especially with American mammals, it also gives biographies and sketches of some of the better known or more noteworthy species of foreign countries. Eifrig includes not only brief descriptions of the mammals and accounts of their habits, but enters into the more important peculiarities of the anatomy and physiology of different species, and their adaptations, and frequently discourses upon economics and conservation. The book is elementary and intended primarily for young people, and particularly for use in nature-study classes. For this purpose the general appeal for conservation is most commendable, but the present reviewer believes it would have been more forceful as such had the illustrations of a trapped coyote (fig. 70) and of a shot giraffe (fig. 146) been omitted. Some of the other illustrations are rather theatrical and border on the fictitious, but if they create in the young reader a real interest in mammalian life their use may be condoned.

In view of the large amount and variety of the information brought together in this volume, there are comparatively few statements open to criticism. We might object to calling the Primates the "first mammals," since they are really the last. Nor do we believe there is evidence that "when the Norway rat came it promptly killed and ate all the black and roof rats which it found in the haunts that were to its liking." We do not approve of classing the sewellel, or mountain beaver, with the beavers, when it is not a beaver, and not even closely related to it. Nor is the hog-nosed skunk ordinarily known as the "hydrophobia skunk," a name that is applied commonly to the spotted skunk. And does the ocelot live on prairie dogs, or the marten on woodchucks? A few other animadversions might glare at the professional mammalogist, but on the whole they are of minor importance.

Eifrig's "Mammals" contains a great amount of information, connectively arranged, and well written. The book is easy on the eyes from both esthetic and utility standpoints. Furthermore, it is well bound and reasonable in price. —HARTLEY H. T. JACKSON.

Tilney, Frederick. THE BRAIN FROM APE TO MAN. New York: Paul B. Hoever, Inc. Large 8vo, vols. 2, pp. xxvii + 1120, figs. 539. Cloth, $25. 1928.

As an account of the comparative anatomy of primate brains Doctor Tilney's work takes its place among those which are conspicuous for elaborateness of description and sumptuousness of illustration. As a contribution to a zoological understanding of man's place in nature it leaves much to be desired. Probably the reason for this contrast is to be sought in the artless mixture of zoological and metaphysical thinking which characterizes the author's mode of approaching his subject, a mixture whose qualities can be appreciated from the following passage, which occurs (p. 729) in the introuuction to Part IV, "From Primitive to Modern Man":

"In the light of history and contemporaneous advancements, it is essential to assume the existence of some specific power which distinguishes man among all living things. But if this assumption disregards the probable genesis of so remarkable a power, it may omit an important detail in the process whereby this striking differentiation has been attained. Were it possible to behold man toiling upward over the long stages of his slow progress during the past half million years or so, this genesis might be the more readily traced. It might be seen in the gradual specialization in those parts of his organization, his hands, with which he has continued to reach out and finally to lay firm hold upon the psyche or soul. This surpassing endowment, Professor Osborn believes, came to man at some critical period when he stood up in the dawning glory of his Cro-Magnon manhood and drew upon the walls of his cave the first imperishable record of his greatness."

Whether my explanation be correct or not, it is difficult to believe that strictly zoological preoccupations could lead Doctor Tilney to the unqualified acceptance (pp. 733–737) of the teeth, calvarium and femur of "*Pithecanthropus*" as parts of a single individual, without specific reference to the writers (notably Ramström, whose name is not even mentioned in the bibliography) who have held contrary views, or to say of the much discussed "*Eoanthropus*" merely that "by some he is regarded as the direct ancestor of *Homo sapiens*, by others he is held to be an independent branch of the human family of quite unknown relations to all other races," without reference to any of the half dozen other and equally contradictory hypotheses to which the Piltdown remains have given rise. No less difficult is it to understand from a zoological point of view the assertion (p. 737) that a primate possessed a "subhuman" face when nothing is known of the creature's skull but the cap down to the eyebrows, or to find the material evidence which would justify the printing (p. 861) of speculations like the following in the form of authenticated history:

"During all the vast era of Pleistocene time, with its recurrent glaciations and intervals of warmth, man's brain steadily grew. This growth at first was slow and irregular. Later it became so decisive as to make cerebral development one of the most striking features in human evolution. The brain slowly increased in volume. It acquired much refinement in many structural details. Its newer parts became more highly specialized. As a result of the evolutional process the brain of modern man is a far more efficient organ than that possessed by the earliest of human kind . . . The ape-like appearance of *Pithecanthropus erectus* affords some reasons for calling this earliest known member of our family circle the Ape-man. The old man of Cro-Magnon, on the other hand, possesses a

nobility of expression which in itself seems to justify his characterization as the 'Paleolithic Greek.' Between these two extremes, the Neanderthal and Piltdown men are intermediate stages."

The most extreme example of zoology led by metaphysical doctrine is furnished (p. 875, italics in the original) by the unqualified declaration—whose chief basis of fact (see fig. 375) is the unusual lumpiness of a small and imperfectly preserved area near the broken edge of an endocranial cast—that:

He ['*Pithecanthropus*'] *had learned to speak*—to communicate in verbal language. The gradual development of skilled acts had eventually combined the effector organs of articulation and phonation into a coördinated apparatus controlled through the brain, by which he was able to express his ideas and feelings. The means of communication thus established laid the foundations of human knowledge."

Particularly at the present time such writing about the evolution of man and the nature of the two supposed "missing links," "*Pithecanthropus*" and "*Eoanthropus*," is something more serious than mere bad zoology. No great acumen is required to enable an interested reader either to detect lacunae and fallacies such as those which I have pointed out, or to apply them, should he be so minded, to advancing the cause of obscurantism, and with telling effect.—G. S. MILLER.

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REPORT OF THE COMMITTEE ON CONSERVATION OF LAND MAMMALS[1]

In this brief report the chairman acknowledges the receipt of material assistance from the other members of the committee. The report may seem to be overly pessimistic. It is our conviction, however, that there has already been entirely too much optimism regarding the status of our wild life. The extinction or serious reduction of species by man has invariably been preceded by widespread belief on the part of the destroyers that such species were so numerous that their extermination was an impossibility. Destructive agencies, either from ignorance or design, are continually preaching this doctrine.

A phase of conservation activity that is particularly popular is the urging of legislation prescribing shorter open seasons for various mammals, especially the so-called fur-bearing species. Then, compliance with the law is considered as representing the fulfillment of all obligations. Without wishing to decry the value due such efforts, we must state our conviction that under present conditions such measures alone are insufficient. It must be apparent that, if the capital stock is being reduced, it makes little difference to the species whether this reduction takes place in one, or in three months. It has repeatedly been pointed out that the principles recognized as necessary for the perpetuation of herds of domestic animals, the killing only of the natural increase, is equally applicable to wild animal stocks. Little progress, however, seems to have been made in the general application of this principle.

The committee has been much impressed by the varying attitudes of the agencies having in charge the administration of game and fur animals. The theory is that the state owns the non-migratory wild life. Without claiming any degree of originality we point out some of the resulting tendencies.

Game commissions, using this term to include all agencies having to do with wild life, not only are short-lived, but have successive varying interests; and in few states, therefore, can any constructive conservation policy enjoy long life. It follows that some states subordinate everything to game, others to fish, often with but vague ideas of the economic relationship of different species or classes of wild life. In some states certain fur-bearing animals are, nominally at least, treated as revenue producers, to be conserved. In other states the same species may be the victims of bounties. In at least one even the chipmunk is classed as a rapacious animal.

There are only a few states where the number of individuals of fur-bearers taken is required of the trapper. Yet if conservation were sincerely desired, this would seem to be the first step to be adopted. If the issuance of a trapping license were contingent on such a report for the previous year being turned in, the returns would be made. Yet the matter is usually treated as unimportant.

In general, the states seem to be indifferent as far as concerns any serious effort to make the annual catch of fur-bearers bear a safe relation to the increase. An example is Idaho, whose law, while requiring trappers to make a report on the catch, gives them ninety days after the close of the trapping season to file such

[1] Report of the committee submitted at the Washington meeting, April 13, 1928.

report. Since the permit for the succeeding season may be granted sixty days after the close of the trapping season, the trapper is under no compulsion to render a report, and usually does not do so. The law is thus admittedly nullified and the fish and game warden does not undertake to make a report of the catch. Yet in the report for the years 1925–26 this optimistic view of the situation is taken. "By a continuance of the present policy there will always be a supply of these animals within our state and an item of real commercial value to those engaged in their capture." The logic of such a conclusion is not apparent.

In a number of states the use of poison in killing predatory and fur-bearing animals is recognized as inimical to wild life in general. Its use in this manner is prohibited in Alaska and the following states: California, Connecticut, Florida, Iowa, Kansas, Maine, Massachusetts, Minnesota (certain exceptions), Missouri, New Hampshire, North Carolina, Pennsylvania, and Vermont. In most of the western states, however, the use of posion is widespread.

In a few states and provinces the rapid decrease of wild life, especially the fur-bearers, is a matter of genuine concern. For example the chief game warden of New Brunswick in his annual report for the fiscal year ending October 31, 1927, says: "If we are to preserve what breeding stock is left some drastic steps will have to be taken—such as—close season on all dead trapped fur-bearing animals for from three to five years." It remains to be seen whether such steps will be taken.

The introduction of mammals for the purpose of supplying game to hunters has reached considerable proportions. In some states, for example, where with reasonable killing rabbits would be abundant, they are now depleted, and are introduced by the thousands from other states. For example, it is thought desirable to furnish cottontail rabbits annually to furnish sport for the miners of Pennsylvania—a state where wild life protection is carried on with special reference to the interests of the sportsmen. This practice has already resulted in the interbreeding of the rabbits of several of our states with foreign stock to a serious extent, and its dangers have frequently been pointed out. The danger of introducing rabbits suffering from tularemia, a disease frequently fatal to human beings, is too serious to be ignored. It is gratifying to note in this connection the efforts of a Michigan organization to discourage this destructive practice. On March 30, 1928, the Michigan Academy of Sciences, Arts, and Letters adopted a resolution disapproving the introduction in the state of all wild animals for stocking purposes, on the ground of the danger to native forms by bringing in diseases, and the general injurious effect, and recommending the enactment of legislation placing the control of all such introductions in the State Department of Conservation.

The inauguration of a comprehensive study of the elk in the Jackson Hole region by the Biological Survey, it is hoped, is the first of several similar much-needed studies. This aggregation, which represents the largest concentration in the United States of any species of large game, presents an unusual opportunity for constructive investigations, and the present attempt should be productive of good results. As a result of close concentration of large numbers of elk on ranch lands in winter a problem has been created that demands solution. The conclusions reached regarding many of the special subjects being investigated, such as diseases and preference for different foods, should be helpful in other sections.

In closing, we wish to say that it is our belief that the salvation of our wild mammals, as has largely been the case with our nongame birds, and is coming to be the case with our game birds, must depend largely on the efforts of that growing class that are interested in wild life for its own sake, and not for profit or the pleasure of killing. If that helpful resource fails to expand fast enough to keep pace with destruction, the future of our wild animals is indeed a gloomy prospect.

[Signed] *T. S. Palmer,*
Lee R. Dice,
Edward A. Preble, Chairman.

REPORT OF THE COMMITTEE ON WILD LIFE SANCTUARIES, INCLUDING PROVISION FOR PREDATORY MAMMALS[1]

The American Society of Mammalogists passed a resolution on April 16, 1924, at the meeting at Harvard University, Cambridge, Massachusetts, authorizing a committee (Journ. Mammalogy, vol. 5, p. 218, 1924) to report to the society on two matters:

1. To formulate policies for the preservation of predatory mammals.
2. To recommend the location of certain wild life preserves suitable for the preservation of such animals.

VALUE OF PREDATORS

1. There is a considerable variety of predators and they are, as a whole, of much scientific, educational and economic value.
2. These animals should be preserved under suitable conditions.
3. The main practical problem is *how* and *where* to do so.
4. The safest general policy with respect to species usually injurious to human interests is intelligent control, rather than wholesale extermination.

POLICIES FOR THEIR PRESERVATION

1. Careful scientific studies of predators regarded as injurious, their ecology and *rôle* in our general economic and social life should be made in *advance* of drastic control measures when practicable.
2. At the present state of our knowledge we are not justified in advocating a total extermination policy for most of our predatory mammals.
3. Policies in regard to dealing with predators, results of control methods, and reasons for adopting them should be made public.
4. Control, rather than extermination policies, are safest, and when certain species are in danger of extermination there should be closed seasons, in appropriate localities, for their protection. Extermination should apply to *certain conditions only*, to be determined by a careful study of the problem.
5. Certain large reservations should give special attention to the preservation

[1] Report of the committee submitted at the Washington meeting, April 13, 1928.

of *large predators*, as many of the smaller kinds are valuable for fur, are easier to preserve and to control, their preservation involves smaller areas, and their supervision is relatively less expensive.

6. That national parks of large acreage, such as the Yellowstone, Yosemite, Glacier, Grand Canyon, Mount McKinley, Crater Lake and Sequoia, and certain national monuments, such as Katmai and Mount Olympus, are today the most suitable areas for the preservation of a few of the larger predators—but *if these areas are not adequate to guarantee the safety of the cougar, certain bears, grizzly and the brown bears, wolverine, timber wolves, and similar large species*, other areas if they can be located, should be set aside deliberately to give certain of these large species additional protection under scientific supervision.

Our knowledge of the predators in national parks is very incomplete. The estimates by Skinner (1927) in the Yellowstone, are the most careful that we know for that park, and those by Grinnell and Storer (1924) are for the Yosemite. These show that the larger predators are far from being safe. Bears are the best appreciated, but even their condition is not satisfactory in spite of their popularity. The wolverine is particularly in danger (Fry, 1923), also the grizzly, fisher, marten and several other species. Our knowledge of the smaller predators in national parks is very limited, and control measures are in the hands of rangers, who are not always experienced naturalists. The smaller national parks, national forests and game preserves can, in some cases, assist greatly in preserving the smaller kinds if a serious effort is made to do so. The larger national monuments are also adapted for these purposes.

The national forests, with over 157,000,000 acres, would at first sight seem an excellent prospect for certain larger predators; and they are for certain smaller kinds. With grazing and game interests paramount in many regions, the larger species must be fully controlled, but many of the smaller ones can be encouraged.

There may be, however, certain remote *wilderness areas* in the national forests, or largely in these forests, that are naturally suitable for the preservation of a limited number of large predators.

There are possibly national forest areas in Alaska, particularly certain *islands*, along the coast, that are suitable for the preservation of certain of the large species; particularly the aquatic and marine ones. Dr. W. T. Shaw suggests that the Glacier Peak region, near the Wenatchee National Forest in central Washington, is a very suitable area for a predatory animal preserve.

7. There may possibly be areas of the public domain in Nevada, Oregon, Washington and Idaho, as well as Alaska, which might be set aside as wild life reservations, where predators and other animals might receive protection and supervision. Your committee has not been able to secure adequate information to recommend specific areas in these regions.

Your committee therefore urgently recommends the preservation of a few predatory animals, and particularly the larger types in the national parks, as fully as is compatible with the welfare of the parks as a whole, and in certain large national monuments, as well as in certain carefully selected wilderness areas if this should prove practicable in the national forests.

With regard to the smaller predators, their preservation is a simpler problem and deserves careful study. An active conservation policy on the part of some of the advanced leaders in the fur industry has been started and can do much

for the conservation of many of the smaller species. A reasonably intelligent study of the conflicting interests of sportsmen interested in vermin control, and the fur industry, deserves a careful scientific investigation by naturalists representing both groups.

Throughout all agencies having to do with the supervision of predatory animals, there should be careful scientific studies with full publication of the results, and all adopted policies should be made public.

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Respectfully submitted,
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Chas. C. Adams, Chairman.

SUPPLEMENTARY REPORT OF THE COMMITTEE ON WILD LIFE SANCTUARIES, INCLUDING PROVISION FOR PREDATORY MAMMALS

At the Philadelphia meeting of the society, in 1923, there was much active discussion and considerable alarm expressed as to the apparent fate of our American predatory mammals. Many causes, including the extension of agriculture, the live stock industry, and the fur industry in its demand for pelts, have combined to make great inroads upon the predatory mammals. The live stock industry has been conducting a campaign for the extermination of certain of these, and active game interests, with "vermin control" campaigns and bounties, have also been active in this destruction. This work has progressed to such a degree that many impartial naturalists have become truly alarmed, because with the refinements of the modern poison warfare that have been devoted to the destruction of these animals, and with the hundreds of thousands of dollars spent annually on the extermination campaigns, the future of certain species looked very serious indeed. Furthermore, this work was being conducted on a large scale, *previous* to what these naturalists considered to be an adequate scientific investigation of the rôle and status of these animals, and also by methods even more scientific than those used in the study of their natural history. It therefore seemed time to stop and inquire just what all this vast expenditure of federal and state funds, aided further by volunteer contributions, was about, and where it was taking us. Were we deliberately progressing or was this just another example of the various states dipping into the federal funds because they were possibly easier to secure than similar amounts from their local legislators? It has also been asked to what degree should public funds be used for protecting any special industry, as that of stock raising?

Allow us to call to mind a few considerations which bear upon this problem. There are differences in detail, but in general these particular points seem worthy of special attention:

1. The desirabitily of preserving *somewhere*, even though in limited numbers, predatory mammals for scientific, educational and economic purposes.

2. The *predatory habit*, in itself, is biologically not wholly harmful. This

habit may, in itself, be a means of controlling rodents and may therefore be of great value in addition to producing a crop of fur.

3. We must recall that predatory mammals include many and varied kinds of animals, for example, sea lions, sea otters, bears, wolves, coyotes, cougars, lynx, bobcats, wolverines, minks, martens, otters, weasels, the domestic dog and cat, ferret, and even man. A predatory mammal policy must recognize all of these predatory habits and behavior in general as a part of this general problem.

4. The influence of predation by mammals is a *relative* one and is not absolute. It may be useful or harmful, depending upon local conditions, and the kinds of animals involved.

5. All the predators, under some circumstances, may be useful to man, and therefore the only sound policy to advocate is a *system of intelligent control suitable for different conditions*, and not a general extermination policy. The startling figures which have been published to show the extent of the damage done by predators appear to be those given by interested parties and do not appear to be the judicious estimates of scientific workers. Possibly this is unavoidable, but great caution is necessary in accepting estimates in such cases because the maintenance of public confidence warrants considerable restraint in such matters.

The Biological Survey of the U. S. Department of Agriculture has not even today, after over three years of public criticism, given us a comprehensive policy for the predatory problem in all its ramifications, showing where the current control methods impinge upon the rodent control problem, the control of rabies, game control, and range management, and where current practices are leading us.

The results of following up various control methods to their logical and practical conclusion are of the greatest public importance. This is a story like that of the "House that Jack Built," and if extermination of the larger predators is practically secure, what about the smaller kinds? If predatory control is overdone, as has been shown recently by Hall (1927) in California, we have an outbreak of mice, and the rodent "poison squad" must be put to work. We have never been fully informed of the relation between predatory mammal control and rodent control. Without a "follow up" system it is possible that many cases like that of the California mice have occurred but have not been recognized. If the rodents are poisoned, what is the next step? Are the predatory animal control forces to function like a police force or a fire department, and be concerned only with *acute emergency conditions*, devoting all its attention to such problems? Or is it as well to deal with the *fundamental scientific study* of the causes and means for their control? What public agency in this field is to concern itself with prevention? Our investigation agencies should not degenerate solely into administrative bureaus in such matters, because the government needs a scientific foundation for its public policies.

With the preceding remarks the public should be equally informed that, in case the federal agency intrusted with investigation and regulation of predacious mammals should be unjustly criticised, the American Society of Mammalogists would immediately rally to its support; and finally, that it is only by its devotion to the public interest that it has undertaken the present chore and subjected itself to possible criticism and misinterpretation in order to improve conditions.

[Signed] *Chas. C. Adams,*
Joseph Dixon,
Edmund Heller.

April 13, 1928.

COMMENT AND NEWS

The Eleventh Annual Meeting of the American Society of Mammalogists will be held at the Museum of Zoölogy, University of Michigan, Ann Arbor, Michigan, from Wednesday, April 10, to Saturday, April 13, 1929, with the meeting of directors during the evening of April 9. Arrangements will be made by a local committee as follows: Lee R. Dice, chairman, Clarence C. Little, William P. Harris, Jr., Alvin G. Whitney, and Peter Okkelberg. The holding of our first middle western meeting is of considerable importance to the society and should prove very stimulating. Because of a change in the time of mailing statements for dues, it is probable that no preliminary announcement of the meeting will be mailed individually to members hereafter, but this will appear in the February number of the JOURNAL.

There has been a noticeable tendency on the part of both conservationists and those engaged in commercializing wild life to ignore the investigations and advice of research workers. The following extracts from an editorial in *The* [London] *Field* of September 13, 1928, have more than indirect application to certain conditions in America.

"A few years ago certain influential medical men acquainted with tropical Africa . . . . urged the wholesale destruction of . . . . game as the sole means of stamping . . . . out sleeping sickness, a disease due to the transference of a living organism from native animals to human beings by the blood-sucking tsetse fly. This proposal . . . . was immediately challenged on the ground that the information available at the time . . . . held no justification for such a drastic and irretrievable step. It was further maintained that such action might lead to results the very opposite of those expected. . . . . A respite, pending further investigations, was happily thus procured. Such investigations have been carried out diligently and impartially, and the result has been complete vindication of the views of the opponents of the proposal, attesting the perspicacity of the naturalists who took up arms in defense of African game.

"Recent evidence of this head is supplied in an article by Mr. Rodney C. Wood on the occurrence of the tsetse fly in Nyasaland, and published in the *Journal of the Society for the Preservation of the Fauna of the Empire*, June, 1928. After reviewing the evidence the author remarks: 'It all seems to point to the conclusion that . . . . as fast as the bigger game is being wiped out so the fly becomes more and more numerous, tends to range further and is more prone to attack man.' The establishment of this truth is perhaps the greatest achievement connected with the society above mentioned, for the ascertained dangerous results of the destruction of big game gives a previously unsuspected utilitarian aspect to its preservation . . . . .

"But in the establishment of game reserves . . . ., one of the first legislative measures to be passed should be the stringent prohibition of the importation

of exotic animals, which over and over again have been shown to have a disastrous effect, often impossible to foresee, upon the native faunas . . . . .

"Convincing and welcome evidence is indeed coming to hand almost daily of the rapid growth of the interest in natural history . . . . . The result of this awakening has been influential and widespread. It has shown itself more particularly in the stirring of an enlightened affection for living animals and of sincere sympathy with them in all parts of the world where the English language is spoken; and it has implanted a realisation of the duty those in authority owe to posterity by taking every possible means to preserve what is left of the native faunas . . . . ."

Dr. R. M. Anderson, of the National Museum of Canada, accompanied the Canadian Arctic patrol to Greenland and the eastern Arctic during last summer. Doctor Anderson had little opportunity for collecting mammals but succeeded in extending the known range of two forms and procured a narwhal skeleton.

In the August issue of this JOURNAL (p. 272), criticism was made of *Parks and Recreation* for copying without credit an article from the JOURNAL OF MAMMALOGY. Both the conductor of the column on conservation in *Parks and Recreation* and the editor of the magazine have expressed regrets for the incident, stated that it was a mistake, and have apologized. This certainly makes all concerned feel better. The JOURNAL OF MAMMALOGY accepts the explanation and apology, and offers *Parks and Recreation* continued coöperation in a good cause.

Charles Sheldon, of Washington, D. C., born at Rutland, Vermont, October 17, 1867, died suddenly at his summer home at Kedgemakooge, Nova Scotia, on September 21, 1928. Mr. Sheldon, a charter member of the American Society of Mammalogists, was well known as an explorer and hunter-naturalist. During the later years of his life he became active in conservation measures, and was one of the prime movers in the National Conference on Outdoor Recreation. Although not a scientist himself, he always took an interest in research work on mammals, and constantly endeavored to stimulate and encourage such investigations. His writings, particularly his book "The Wilderness of the Upper Yukon," are not only expositions of the wild life of the regions he visited, but contain actual contributions to scientific knowledge. Mammalogy has lost another friend.

Dr. Peter P. Sushkin, curator of birds in the Zoölogical Museum, Academy of Sciences, Leningrad, Union of Socialistic Soviet Republics, died of pneumonia at Kislovodskaia, Northern Caucasia, on September 29, 1928. An ornithologist by preference and profession, he nevertheless had a deep interest in mammals. Doctor Sushkin visited the United States three years ago, and will long be remembered by many of us not only as a keen scientist, but as a jovial and kindly man.

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